Who Benefits from Benefits?
Municipal Consequences of Federal Cash Transfers

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Abstract

Centralized welfare programs limit local discretion over eligibility and payments, yet access often depends on municipal bureaucracies. We study whether local politicians obtain electoral returns from a federally designed cash transfer when their remaining discretion lies in registration. In Brazil's *Bolsa Família*, a 2009 reform unexpectedly changed the formula assigning municipal beneficiary quotas, and a later federal revision opened additional slots during the same mayoral term. We measure the cumulative authorization change relative to the allocation implied by the previous rule. In the 2,078 municipalities where the mayor elected in 2008 sought reelection in 2012, a one-standard-deviation quota shock created 171.50 additional authorized slots per 10,000 residents and induced 54.68 additional beneficiary families per 10,000 residents. The 2SLS estimate implies that 100 additional beneficiary families per 10,000 residents raise vote share by 2.59 percentage points. Enrollment expanded both with and without a pre-existing local welfare center (*CRAS*), but we find a clear electoral response only in *CRAS* municipalities. Within that group, the vote-share response is larger where offices report active outreach and more intensive service to *Bolsa Família* families. The manager turnover evidence points in a consistent direction. The size of the quota shock does not predict manager turnover, and substitute managers do not appear to have been prior partisan loyalists. We interpret these patterns as scope conditions for the local implementation channel. Among municipalities with turnover, however, larger shocks predict a higher probability that the substitute later runs for office. Substitute managers are also more likely to run than managers who stayed and the outgoing managers they replaced. Centralizing eligibility and payment does not eliminate local political returns when program access still depends on municipal implementation.

Keywords: cash transfers; electoral returns; state capacity; *Bolsa Família*.
JEL Codes: D72, H53, H75, I38, O17.

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1 Introduction

Across the developing world, cash transfer programs have become a leading tool for reducing poverty while insulating redistribution from local political discretion. By standardizing eligibility rules, fixing benefit values by federal decree, and processing payments through centralized banking systems, these programs are designed to reduce the scope for patronage and discretionary allocation (Fiszbein and Schady, 2009). Yet access often depends on local bureaucracies controlled by elected officials. Can local politicians benefit electorally from policies designed and financed by another level of government? This is the question we address.

We study this question in the context of Brazil's *Bolsa Família*, one of the largest conditional cash transfer programs in the world.[1] Although eligibility rules, benefit size, and payments are determined by the federal government, municipalities remain responsible for identifying and registering eligible households, mostly through local welfare centers known as *Centros de Referência de Assistência Social* (*CRAS*). This institutional arrangement creates a narrow but important margin of local discretion: while mayors cannot alter transfer amounts, redirect funds, or withhold payments, they control the administrative pipeline through which households may gain access to the program.

This margin depended on a registration process that required active municipal effort. Identifying potentially eligible families meant locating them, conducting household interviews, and collecting documentation; municipal authorities controlled the local registration apparatus, which gave them influence over the pace, intensity, and reach of enrollment efforts.[2] In practice, two municipalities facing similar levels of underlying need could generate very different realized coverage depending on how actively the local administration pursued registration.

Our empirical strategy starts from a 2009 reform that revised the formula used to compute municipal *Bolsa Família* quotas. The program operates through federally determined municipal quotas, which set a ceiling on the number of families that can be served in each municipality.[3] A mayor elected in October 2008 could expect the

[1] By June 2006, *Bolsa Família* covered 11.10 million families, approximately 46 million people, or about 25% of the Brazilian population. By June 2015, the program supported 13.80 million families, with an average monthly benefit of R$167.15 and a budget equivalent to 0.50% of GDP.

[2] Hellmann (2015) describes enrollment as a municipal responsibility. He reports that as of September 2014, 77% of registrations were made without a home visit. Municipalities were responsible for preparing the local structure for registration, training personnel, promoting awareness campaigns, and designing *active search* strategies to reach eligible families outside the existing registry.

[3] Lindert et al. (2007) explains that *Bolsa Família* maintained municipal program quotas based on local

next adjustment to follow the previous allocation rule. Instead, the federal government changed that rule in 2009 and revised municipal quotas again in 2012.[4] These revisions expanded the ceiling, not the registry. Families who were already registered could enter when new slots opened, while filling the remaining slots depended on local governments' effort to identify, locate, and register eligible families not yet in the federal database.

We combine administrative data on quotas, realized beneficiary counts, and local social-assistance infrastructure with municipal electoral data from the subsequent election. The construction of the 2009 benchmark follows Gerard et al. (2025): we apply the previous allocation rule to the information available before the reform. Our main measure subtracts that benchmark from the official 2012 quota, capturing the cumulative authorization that opened during the mayor's term. We use this quota change to instrument realized beneficiary expansion.

Our findings trace the reform along four stages: the relationship between federal authorization and realized enrollment; the electoral response to the authorization shock; the 2SLS estimate that combines the two; and the institutional conditions under which these patterns emerge.

The first stage quantifies how federal authorization translated into realized enrollment. A one-standard-deviation increase in the quota shock corresponds to 171.50 additional authorized beneficiary-family slots per 10,000 residents. It raises realized beneficiary expansion by 0.30 standard deviations, equivalent to 54.68 additional beneficiary families per 10,000 residents.

Once federal authorization is shown to translate into realized enrollment, we estimate the electoral return to expanding the ceiling itself, before separating out how much of that ceiling municipalities chose to fill. For the same one-standard-deviation quota shock, the reduced-form intention-to-treat estimate is 1.41 percentage points on the incumbent's vote share. This is the electoral consequence of additional federal authorization, integrating over the local enrollment response documented in the first stage.

Scaling the intention-to-treat effect by the first stage gives the electoral response

poverty estimates. Original municipal allocations were constructed by combining the 2001 PNAD with the 2000 Census, generating an initial national target of 11.20 million families when the program was created in 2003.

[4] Hellmann (2015) notes that *Bolsa Família* administered municipal quotas using estimates of poor families developed in partnership with IBGE and reports that, in 2009, the federal government refined the measurement of the target population by incorporating methodologies to capture income volatility among vulnerable households. These studies formed a major expansion of coverage, incorporating more than 1.60 million additional families.

to realized beneficiary expansion. A one-standard-deviation increase in instrumented enrollment, about 181.36 additional beneficiary families per 10,000 residents, raises incumbent vote share by 4.69 percentage points. Equivalently, an increase of 100 beneficiary families per 10,000 residents raises vote share by 2.59 percentage points. The estimate is similar across alternative quota definitions, outcome definitions, and corrections for selection into the rerunning sample.

These returns differ across the institutional layer that converts authorization into delivery. Of the 2,078 municipalities in the estimation sample, 1,394 had a pre-existing local welfare center in 2008 and 684 did not. Realized enrollment expands in both groups: the first-stage coefficients are 0.30 with a *CRAS* and 0.32 without one. The divergence is electoral. The corresponding 2SLS estimate is 6.11 in the *CRAS* group; outside it, the confidence set includes zero. Pre-existing infrastructure therefore does not determine whether authorization becomes enrollment; it conditions where realized expansion generates a clear electoral return.

Within these municipalities, we next examine two measures of how the local welfare apparatus operated. The first records whether municipal teams proactively searched for potentially eligible families (active outreach) rather than waiting for them to appear at a registration point. The first stage remains positive both with and without this practice, while the reduced-form vote-share point estimate is 2.00 percentage points with active outreach and 0.76 without it. The second measure records whether the *CRAS* reported serving *Bolsa Família* families through PAIF (*Serviço de Proteção e Atendimento Integral à Família*), a continuing family-support service provided through the social-assistance system. The corresponding reduced-form estimates are 2.25 percentage points where at least one *Bolsa Família* family was served through PAIF and 0.65 where none was reported. The PAIF contrast becomes sharper when we consider service volume: among municipalities reporting at least one family served through PAIF, the reduced form is 5.36 above the median number of families served and indistinguishable from zero below it. Because both operational measures predate the quota shock but were not randomly assigned, we interpret these results as scope patterns. They are consistent with stronger political returns where the local delivery apparatus undertook more active outreach and provided more intensive family support.

A second margin opens around the front-line manager who runs the unit day-to-day. We observe a manager change in 1,076 municipalities and no observed change in the remaining 318; the latter group includes municipalities without a matchable two-wave manager status. The first-stage estimates are similar across the two groups: 0.31 with observed turnover and 0.26 without it. The corresponding vote-share 2SLS

estimates are 5.92 and 3.06, although the difference between them is imprecisely estimated. We find no evidence that the shock itself predicts manager turnover or new *CRAS* openings, or that replacements were prior partisan loyalists. Their distinctive response is electoral: external incoming managers run for office at roughly twice the rate of stayers, while a one-standard-deviation quota shock is associated with an increase in the probability that a substitute later runs.

These findings advance four related literatures. We speak first to the political economy of cash transfer programs. A large body of work shows that incumbents benefit electorally from the expansion of social transfers (Manacorda et al., 2011; De La O, 2013). Much of this literature studies settings in which local discretion is deliberately minimized, relying on centrally determined thresholds, randomized assignment, or other exogenous variation to identify electoral effects. We complement this work by studying a setting in which the program architecture is centralized but the last mile of delivery is not. Our findings suggest that centralizing transfer rules and payment flows is not sufficient to depoliticize redistribution when access to the program depends on a locally controlled registration bureaucracy. Even when mayors cannot alter eligibility criteria or benefit levels, control over the administrative pipeline through which households gain program access can still generate measurable electoral returns.

Our paper is especially closely related to Frey (2019), but the two designs study electoral returns under different implementation regimes: Frey's intervention leaves less room for mayoral involvement, whereas ours depends directly on municipal administration. Frey exploits a discontinuity in federal funding for the *Family Health Program*, a primary-care program in which teams follow a federally specified protocol of routine home visits to poor households. His information channel is embedded in those visits: the teams disseminate *Bolsa Família* eligibility information as part of their routine work, making enrollment less dependent on a separate effort by the mayor's welfare administration. We study the complementary margin. In our setting, converting expanded federal authorization into realized enrollment requires municipal welfare staff to identify, locate, and register eligible families. The designs also draw variation from different parts of the municipal distribution. Frey's threshold design is concentrated among small, low-development municipalities near a funding threshold, whereas the quota reform created more room for expansion in larger municipalities that had not yet absorbed earlier increases. Within his sample, coverage growth through a federally structured outreach channel reduces incumbency advantage and shifts the composition of challengers toward less clientelistic parties. Within ours, coverage growth through a locally managed registration channel produces positive returns for the in-

cumbent.

Second, we contribute to the literature on political accountability under shared governance. A central question in this literature is whether voters can attribute responsibility when multiple levels of government jointly produce policy outcomes (Przeworski et al., 1999; Besley, 2006). Ferraz and Finan (2008) show that voters punish corrupt mayors when randomly assigned audits reveal misuse of federal funds, especially where local media facilitate information transmission. Avis et al. (2018) show that audits also reduce corruption through a legal disciplining channel. Our setting differs because the relevant outcome is positive delivery rather than malfeasance. The results are consistent with voters giving local incumbents some credit for the expansion of a federally funded program when access still depends on municipal action. We do not observe voters' beliefs directly, but the pattern fits an attribution channel in a setting of shared responsibility.

Third, we contribute to the literature on state capacity and the bureaucratic foundations of policy delivery. Besley and Persson (2009, 2011) establish state capacity as a precondition for credible policy delivery, and Finan et al. (2017) survey the personnel and bureaucratic infrastructure through which public programs are implemented. Our findings distinguish enrollment pass-through from political conversion. Municipalities both with and without a pre-existing *CRAS* translate the quota shock into additional beneficiaries. The electoral response, however, is positive only where the delivery apparatus was already present. Local infrastructure therefore conditions the political return to program expansion, even though it is not required for aggregate enrollment to respond.

Fourth, conditional on the presence of a delivery apparatus, our within-*CRAS* findings on bureaucratic turnover speak to the literature on the selection and politicization of frontline bureaucrats. Manager turnover is tied to the 2008 mayoral transition, which takes office months before the 2009 reform. Yet the substitutes those mayors install do not appear to be selected for prior partisan alignment. Two readings are consistent with what these substitutes do once installed. Program expansion may raise the attractiveness of the managerial post and draw a more ambitious pool of candidates into it, consistent with Dal Bó et al. (2013). The post may also become a launching pad: external substitutes run for elected office at roughly twice the rate of stayers, while a one-standard-deviation quota shock is associated with an increase in subsequent candidacy. This pattern is close to Frey and Santarrosa (2026), who show that voter-facing bureaucrats involved in *Bolsa Família* can accumulate political capital as the program expands. It differs from prior partisan selection in Colonnelli et al. (2020) and from

hiring-discretion turnover in Akhtari et al. (2022): here the clearest political response appears after appointment.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting of *Bolsa Família*, the quota system, the federal revisions, and the data sources. Section 3 presents the empirical strategy, defines the instrument, and discusses the identifying assumptions. Section 4 reports the main results, robustness checks, and heterogeneity. Section 5 examines the implementation conditions and the subsequent careers of substitute managers. Section 6 concludes.

2 Background and Data

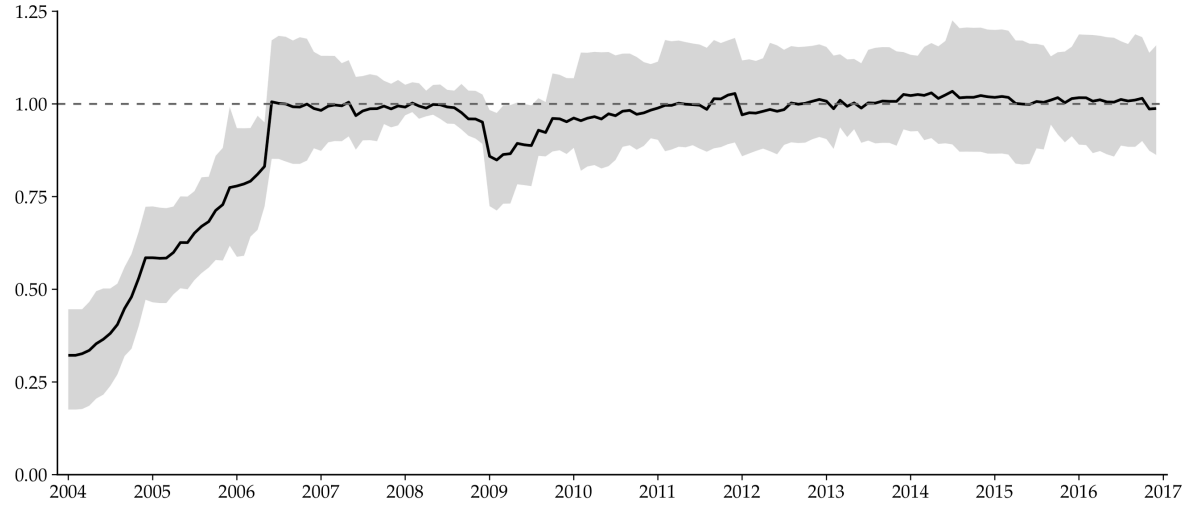
2.1 Institutional Background

Launched in 2004, *Bolsa Família* consolidated several pre-existing social assistance programs into a single conditional cash transfer system, becoming Brazil's flagship poverty alleviation policy. In 2009, the program classified eligible families into two income groups. Families with monthly per capita income at or below R$70 were classified as extremely poor and received a basic monthly benefit of R$68, plus variable benefits of R$22 per child or adolescent aged 0–15, up to three, and R$33 per adolescent aged 16–17, up to two. Families with monthly per capita income above R$70 and at or below R$140 qualified only for the child- and adolescent-linked variable benefits. These benefits were conditional on age-specific education and health requirements. The maximum monthly transfer per family was R$200. By December 2009, the program reached approximately 12.40 million families, with an average benefit of about R$95.

Central to the program's operations is the *Cadastro Único* (*CadÚnico*), a national registry of low-income households that serves as the gateway to *Bolsa Família* and dozens of other social policies. The registry targets families with per capita income below half the minimum wage (R$232.50 in 2009), a threshold substantially higher than the program's own eligibility lines, so that the pool of registered families considerably exceeds the set of actual beneficiaries.

Eligibility criteria are nationally uniform, benefit values are fixed by federal decree, and payments are processed and deposited directly into beneficiaries' accounts by the state-owned bank *Caixa Econômica Federal*. Mayors cannot alter transfer amounts, redirect funds, or withhold payments. What municipal governments control is the registration process itself.

Figure 1: Realized Enrollment as a Share of Authorized Quota



Note. For each municipality-month, we compute the ratio of realized *Bolsa Família* beneficiaries to the authorized quota. The solid line plots the quota-weighted cross-municipal mean of these municipality-level ratios. The shaded band spans the weighted 25th to 75th percentile. The dashed horizontal line marks full take-up (ratio = 1). Before 2007, municipalities operated well below their authorized ceilings, and the rapid convergence toward 1 reflects the administrative build-up of the program in its first years. After this initial convergence, realized enrollment generally tracked authorized capacity closely. The 2009 reform temporarily broke that alignment: expanded quotas opened a gap between authorized slots and realized beneficiaries, creating the local implementation margin through which municipalities had to identify, register, and enroll additional eligible families.

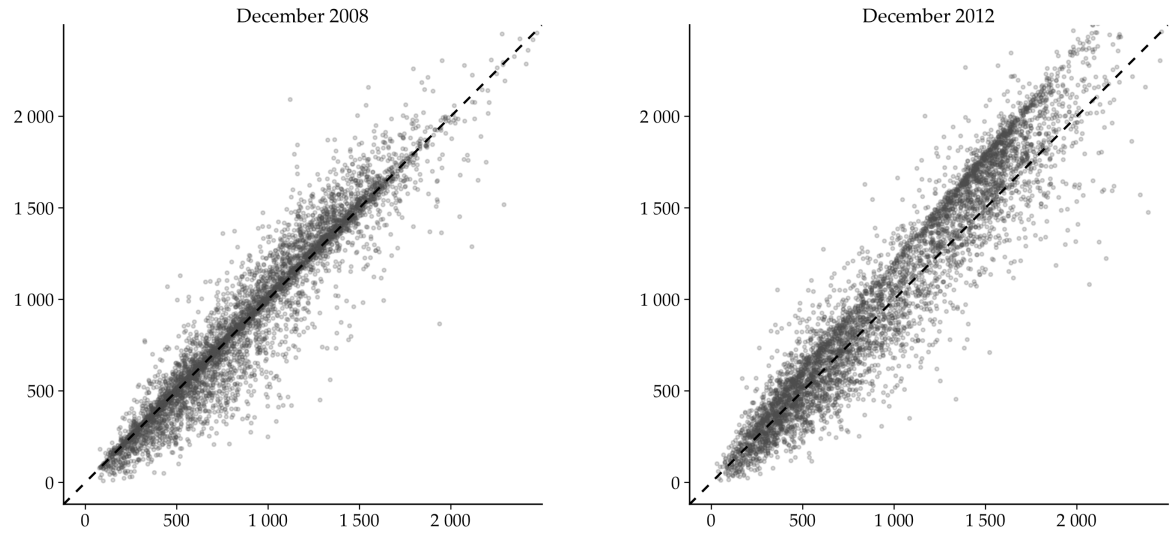
Registration in the *Cadastro Único* requires a face-to-face interview: a social worker fills out a standardized questionnaire covering household composition, housing conditions, assets, employment, and income. In urban areas, families typically present themselves at a *Centro de Referência de Assistência Social* (*CRAS*). In rural and remote areas, however, eligible families cannot realistically be expected to travel to a registration point. There, coverage depends on active outreach: municipal teams must physically locate potential beneficiaries, travel to communities with limited road access, no telephone coverage, and no internet, and conduct household interviews on site. Federal regulations encourage this practice by recommending that interviews take place at the family's home, both to facilitate verification and to reach populations that would otherwise remain invisible to the registry.[5]

Eligibility alone does not guarantee benefit receipt. Access to *Bolsa Família* depends on the availability of municipal quotas: caps on the number of beneficiaries per municipality, defined nationally and revised approximately every three years based on poverty estimates derived from census and survey data. These quotas function as of-

[5] In 2009, there were approximately 5,800 *CRAS* units distributed across roughly 4,000 of Brazil's 5,568 municipalities.

ficial upper bounds on municipal enrollment, but the constraint is not always immediately binding. In practice, municipalities frequently operate near their ceiling, oscillating modestly above or below it from month to month before the federal priority mechanism reallocates unused slots.

Figure 2: Authorized Quota vs. Realized Enrollment

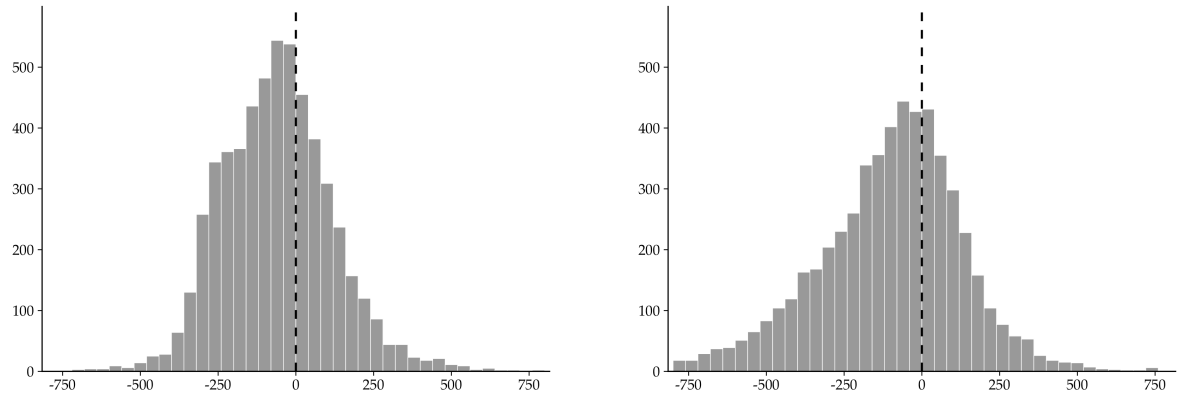


Note. Each dot is a municipality. The horizontal axis shows the federally authorized quota and the vertical axis shows the 12-month average stock of families enrolled in *Bolsa Família*, both per 10,000 residents using fixed 2006 population. The dashed line marks the 45-degree locus of full take-up (realized = authorized). The panels show December 2008, before the mayoral term, and December 2012, at its endpoint. Both axes share a cap at the 99th percentile of the displayed cross-municipal distributions (approximately 2,400 beneficiary families per 10,000 residents); observations above this threshold are excluded from the frame but not from any analysis.

The priority index considers both quota utilization and unmet registered demand: municipalities with low beneficiary-to-quota ratios and few eligible-but-unserved families risk seeing unused slots reallocated to places with stronger registered demand. The quota system therefore creates a use-it-or-lose-it implementation pressure. A quota increase is not a benefit guarantee, but it opens federally authorized room that municipalities must fill by identifying, registering, and updating eligible households. Effective coverage thus depends on local administrative initiative: political will and organizational capacity determine how much of the authorized ceiling becomes actual benefit receipt.

In 2009, the federal government revised the methodology used to compute municipal quotas, producing a heterogeneous reallocation of authorized beneficiary slots across municipalities. The previous approach, in place since 2006, distributed slots in proportion to each municipality's estimated number of poor families from the 2000 Census, adjusted for intercensal population growth. The 2009 revised methodology

Figure 3: Distribution of Slack Between Authorized Quota and Realized Enrollment



Note. Each panel shows slack in December 2012 per 10,000 residents using fixed 2006 population. Slack is the relevant authorized quota minus the 12-month average stock of beneficiary families in 2012. The left panel uses the official 2012 quota; the right panel uses the counterfactual 2009 quota implied by the previous allocation rule. Positive values indicate authorized room not translated into realized enrollment, while negative values indicate realized enrollment above the relevant quota. The dashed vertical line marks zero slack. Both panels retain the common display window from -800 to 800 beneficiary families per 10,000 residents to limit the influence of extreme observations.

accounts for income volatility among families near the eligibility line, a feature of transient poverty that earlier projections systematically undercount (Gerard et al., 2025).

Alongside the methodological update, all municipal quotas were scaled by a factor of 1.18 to account for structural income volatility near the poverty threshold. Because this multiplicative factor is constant, it contributes no cross-municipal variation to the instrument; all identifying variation in the quota shock derives from the updated municipal poverty estimates and the change in income thresholds. Together, these changes added nearly 1.60 million new authorized slots in 2009. The reform expanded the ceiling, not the registry: although already-registered eligible families were absorbed automatically, the magnitude of the expansion left a large residual margin of unfilled slots whose realization depended on the municipal government's effort to identify, locate, and register eligible families not yet in the federal database.

2.2 Data Sources

Our empirical analysis combines four municipal-level data sources: administrative records on *Bolsa Família*, electoral returns, census and household-survey data, and administrative records on local social-assistance infrastructure.

We begin with monthly administrative records from the *Ministério do Desenvolvimento Social* on *Bolsa Família* quotas, realized coverage, and total transfers from 2006

to 2012. These data span the pre-reform baseline, the 2009 reallocation, and the post-reform expansion window through the 2012 election, and allow us to measure both authorized and realized program expansion.

We then use electoral records from the *Tribunal Superior Eleitoral* (*TSE*) for the 2008 and 2012 municipal elections. These data provide candidate-level vote totals, turnout, and detailed candidate characteristics, including education, prior occupation, and party affiliation.

To reconstruct counterfactual municipal quotas under the pre-reform allocation rule, we combine the 2000 Census from the *Instituto Brasileiro de Geografia e Estatística* (*IBGE*) with poverty estimates from the 2006 *Pesquisa Nacional por Amostra de Domicílios* (*PNAD*). These are the inputs used to approximate the quota formula that would have prevailed without the 2009 reform.

Finally, we use administrative records from the *Censo SUAS* covering the universe of *CRAS* units in 2008 and 2009. In addition to information on the identity and tenure of directors and staff, these data include measures of local infrastructure and service provision, such as whether units conduct interviews and active searches.

2.3 Descriptive Statistics

The estimation sample consists of 2,078 Brazilian municipalities observed in the 2008 and 2012 elections in which the mayor elected in 2008 sought reelection in 2012. Table 1 reports descriptive statistics for the filtered municipality universe and for this exact regression sample.

Three features stand out. The 2008 winner's vote share averages 54.97 percent in the broader municipality sample and 52.92 percent in the regression sample. Predetermined municipal characteristics, including urbanization, poverty, income, and inequality, have similar means across the two panels. The characteristics of the elected mayors differ more, which is expected because the regression sample requires the 2008 winner to seek another term in 2012. Finally, both authorized and realized *Bolsa Família* expansion are right-skewed, so the regressions report changes in standard-deviation units and translate the main estimates back into families per 10,000 residents.

Table 1: Descriptive Statistics

Panel A: All Municipalities							**Panel B: Regression Sample**						
	N	Min	Mean	Median	Max	SD		N	Min	Mean	Median	Max	SD
Population (2000)	4,976	795.00	31,515.59	10,703.50	10,277,518.00	190,735.22	Population (2000)	2,078	795.00	27,991.14	10,556.50	5,773,130.00	146,177.39
Urban Population (2000) (%)	4,976	0.00	59.29	59.92	100.00	23.32	Urban Population (2000) (%)	2,078	0.00	58.80	59.35	100.00	23.32
Poor Population (2000) (%)	4,976	5.17	64.02	66.23	99.00	20.76	Poor Population (2000) (%)	2,078	7.66	64.09	66.80	99.00	20.99
Income per capita (2000) (R$)	4,976	62.65	338.27	306.00	1,759.76	193.29	Income per capita (2000) (R$)	2,078	69.20	337.40	300.13	1,759.76	195.41
Gini (2000) (%)	4,976	30.00	54.78	55.00	82.00	6.80	Gini (2000) (%)	2,078	30.00	54.84	55.00	80.00	6.73
Formal Workers (2000) (%)	4,976	1.92	36.15	34.44	86.38	18.16	Formal Workers (2000) (%)	2,078	2.89	35.80	33.98	86.38	18.31
Permitted *Bolsa Família* Families (2008)	4,976	24.00	2,078.06	917.50	285,060.00	6,980.68	Permitted *Bolsa Família* Families (2008)	2,078	24.00	1,898.41	906.00	164,913.00	5,077.26
Permitted *Bolsa Família* Families (2008) (%)	4,976	0.77	8.81	8.29	67.43	4.66	Permitted *Bolsa Família* Families (2008) (%)	2,078	0.77	8.89	8.39	33.00	4.71
Permitted *Bolsa Família* Families (2008) per 100 Families	4,976	2.89	34.32	30.33	113.31	18.66	Permitted *Bolsa Família* Families (2008) per 100 Families	2,078	2.89	34.60	31.12	110.30	18.94
Permitted *Bolsa Família* Families (2008) per 10,000 Residents	4,976	77.08	880.65	829.13	6,742.82	466.41	Permitted *Bolsa Família* Families (2008) per 10,000 Residents	2,078	77.08	888.88	839.23	3,299.89	470.55
Δ Permitted *Bolsa Família* Families per 100 Families	4,976	-36.28	5.59	3.48	103.48	11.72	Δ Permitted *Bolsa Família* Families per 100 Families	2,078	-36.28	5.71	3.57	86.01	11.74
Δ Permitted *Bolsa Família* Families per 10,000 Residents	4,976	-2,841.24	116.55	91.84	2,865.11	274.55	Δ Permitted *Bolsa Família* Families per 10,000 Residents	2,078	-1,000.62	120.42	95.75	2,865.11	277.26
Realized *Bolsa Família* Families (2008)	4,976	2.42	2,045.08	914.79	204,410.17	5,990.93	Realized *Bolsa Família* Families (2008)	2,078	5.50	1,871.27	908.38	133,322.75	4,543.18
Realized *Bolsa Família* Families (2008) (%)	4,976	0.08	8.76	8.13	61.49	4.91	Realized *Bolsa Família* Families (2008) (%)	2,078	0.26	8.80	8.31	29.64	4.90
Realized *Bolsa Família* Families (2008) per 100 Families	4,976	0.28	34.49	30.03	105.92	20.21	Realized *Bolsa Família* Families (2008) per 100 Families	2,078	0.93	34.58	30.84	89.54	20.26
Realized *Bolsa Família* Families (2008) per 10,000 Residents	4,976	8.48	876.40	813.07	6,149.19	491.21	Realized *Bolsa Família* Families (2008) per 10,000 Residents	2,078	25.81	879.87	830.99	2,963.95	490.46
Δ Realized *Bolsa Família* Families per 100 Families	4,976	-13.85	7.78	5.26	93.70	10.06	Δ Realized *Bolsa Família* Families per 100 Families	2,078	-12.71	8.63	6.09	93.70	10.28
Δ Realized *Bolsa Família* Families per 10,000 Residents	4,976	-1,234.13	180.99	139.04	2,979.12	231.70	Δ Realized *Bolsa Família* Families per 10,000 Residents	2,078	-398.59	203.50	163.15	2,979.12	238.38
Candidates (2008)	4,976	2.00	2.73	2.00	12.00	1.02	Candidates (2008)	2,078	2.00	2.80	3.00	12.00	1.07
Candidates (2012)	4,976	2.00	2.77	2.00	12.00	1.08	Candidates (2012)	2,078	2.00	2.70	2.00	9.00	1.00
Trying Reelection (2012) (%)	4,976	0.00	43.75	0.00	100.00	49.61	Trying Reelection (2012) (%)	2,078	100.00	100.00	100.00	100.00	0.00
Winner's Vote Share (2008) (%)	4,976	24.06	54.97	53.73	99.98	10.46	Winner's Vote Share (2008) (%)	2,078	24.06	52.92	52.68	99.16	9.43
Incumbent Vote Share (2012) (%)	2,177	0.00	48.28	49.24	100.00	16.41	Incumbent Vote Share (2012) (%)	2,078	0.18	49.99	49.65	100.00	14.03
Δ Incumbent Vote Share (pp)	2,177	-79.46	-4.60	-3.60	55.47	17.29	Δ Incumbent Vote Share (pp)	2,078	-71.82	-2.94	-3.03	55.47	15.13
Female Mayor (2008) (%)	4,970	0.00	9.22	0.00	100.00	28.93	Female Mayor (2008) (%)	2,078	0.00	10.15	0.00	100.00	30.21
High School or More (2008) (%)	4,970	0.00	78.85	100.00	100.00	40.84	High School or More (2008) (%)	2,078	0.00	81.14	100.00	100.00	39.13
College Degree (2008) (%)	4,970	0.00	45.11	0.00	100.00	49.77	College Degree (2008) (%)	2,078	0.00	46.39	0.00	100.00	49.88
Mayor Age (2008)	4,970	21.00	48.16	48.00	90.00	9.52	Mayor Age (2008)	2,078	21.00	46.51	46.00	81.00	9.08
Married (2008) (%)	4,970	0.00	78.13	100.00	100.00	41.34	Married (2008) (%)	2,078	0.00	77.05	100.00	100.00	42.06
Public-Sector Occupation (2008) (%)	4,970	0.00	34.21	0.00	100.00	47.44	Public-Sector Occupation (2008) (%)	2,078	0.00	15.74	0.00	100.00	36.42
Agricultural Occupation (2008) (%)	4,970	0.00	12.03	0.00	100.00	32.54	Agricultural Occupation (2008) (%)	2,078	0.00	14.49	0.00	100.00	35.20
Business Occupation (2008) (%)	4,970	0.00	20.24	0.00	100.00	40.18	Business Occupation (2008) (%)	2,078	0.00	26.32	0.00	100.00	44.05

Notes: Each observation is one municipality. Panel A contains municipalities with a balanced 2008–2012 election panel that satisfy the common election-quality restrictions; *N* is the number with nonmissing values in each row. Panel B is the exact locked regression sample used in the headline estimates (2,078 municipalities). Permitted families in 2008 are the pre-reform authorized quota (EST 2006); realized families are 12-month averages. Percentage and per-10,000 measures use fixed 2006 population, while per-100-family measures use Census-2000 families. Every Δ is the corresponding 2012 value minus its 2008 value. Candidate counts describe first-round mayoral candidates. Trying reelection indicates that the sitting mayor contested the 2012 election. Vote share tracks the 2008 winner: the 2008 value is that winner's baseline share, and the 2012 value and its change are observed only when the same mayor reran. Mayor characteristics describe the 2008 winner and match the headline controls; age squared is omitted because it is a deterministic transformation of age.

3 Empirical Strategy

Our empirical strategy exploits federal revisions to the municipal *Bolsa Família* quotas during the 2009–2012 mayoral term. These revisions changed how much each municipality was authorized to expand, while actual enrollment still depended on local registration. We compare the official quota with the allocation implied by the previous rule and use that difference to instrument realized beneficiary expansion. The two-election design compares changes in electoral performance; identification comes from the claim that, conditional on pre-reform characteristics, the federal quota change was unrelated to the municipality's underlying electoral trend.

3.1 Federal Quota Revisions as a Source of Identification

The *Ministério do Desenvolvimento Social* (MDS) set municipal quotas nationally using census and survey data. Municipal governments could not choose their own quotas. Brollo et al. (2020) find evidence of political manipulation in the enforcement of program conditions, but not in quota allocation.[6]

To isolate the variation attributable solely to the change in allocation rules, we construct counterfactual 2009 quotas by applying the pre-reform methodology, which had been used to set the 2004 and 2006 quotas, to the data that would have been available immediately before the reform,

$$\text{CountQuota}_m^{2009} = \frac{\text{Poor}_m^{2000} \cdot n_m^{[2000,2006]}}{\sum_{k \in s} \text{Poor}_k^{2000} \cdot n_k^{[2000,2006]}} \cdot 1.18 \cdot \text{Poor}_s^{2006} \tag{1}$$

where Poor_m^{2000} is the number of poor families in municipality m of state s from the 2000 Census, $n_m^{[2000,2006]}$ is an IBGE estimate of municipal population growth between 2000 and 2006, Poor_s^{2006} is the state-level estimate of poor families from the 2006 *Pesquisa Nacional por Amostra de Domicílios* (PNAD), and the factor 1.18 reflects a uniform expansion applied to all municipal quotas as part of the reform.[7] The actual 2009 quotas were computed by MDS using a revised methodology that incorporated updated poverty es-

[6] Gerard et al. (2025) cannot reproduce the official MDS quota numbers exactly from the public information and the pre-reform formula. They do not read this gap as evidence of political manipulation. It may reflect unpublished administrative adjustments or differences in the auxiliary estimates used by MDS.

[7] The last officially reported quota update before the 2009 reform used information from the 2000 Census and the 2004 PNAD. To construct the counterfactual, we update the pre-reform formula using 2006 PNAD data, which is the most recent information that would have been available to policymakers at the time, while holding the allocation rule fixed.

timates and new income-volatility adjustments, and the exact formula is not publicly available. The preferred instrument is therefore

$$\Delta \text{Quota}_m^{2012} = \frac{\text{Quota}_m^{2012} - \text{CountQuota}_m^{2009}}{\text{Population}_m^{2006}} \tag{2}$$

For presentation and estimation, we multiply this rate by 10,000. This measure captures the cross-municipal variation in authorized expansion arising from the change in the federal allocation methodology, net of the uniform national expansion and local poverty differences. In the preferred specification, the counterfactual quota counts as poor any household with monthly per-capita income below R$90 in the 2000 Census and below R$137 in the 2006 PNAD. The R$137 cutoff is the eligibility threshold announced by MDS *Informe* nº 169 as part of the 2009 reform itself.

Subtracting the formula-based counterfactual separates the new authorization from the allocation that the previous rule would have produced (Borusyak and Hull, 2023).[8]

The mayor elected in 2008 entered office under the previous allocation rule. The federal government then changed the formula in 2009 and revised municipal quotas again in 2012. Neither revision was set by the mayor, and the first was announced only after the new administration had taken office. Municipalities could register eligible households in the *Cadastro Único* throughout the term; when new slots opened in 2012, families already in the registry could be incorporated automatically. Our main measure therefore treats the official 2012 quota minus the counterfactual 2009 quota as cumulative authorization over the term. The construction of the counterfactual follows Gerard et al. (2025); extending the endpoint to 2012 is ours and matches the 2008–2012 enrollment outcome. We also report a short window that uses the 2009 authorization change and the change between the twelve-month enrollment averages for 2008 and 2009.

3.2 Research Design

The design compares changes in incumbent electoral performance between 2008 and 2012 across municipalities that faced different amounts of authorized and realized expansion. Because expansion is continuous, the 2SLS coefficient summarizes the marginal electoral response among municipalities whose enrollment responds to the federal quota change (Angrist and Imbens, 1995). Three assumptions carry the

[8]Because the R$137 eligibility line was introduced jointly with the 2009 expansion, we also build alternative versions using the pre-reform R$120 line.

argument, and we discuss each below.

The primary challenge is the endogeneity of realized enrollment. Mayors influence how many families are actually registered, so coverage growth reflects local political incentives. Municipalities where incumbents face stronger electoral pressures may expand coverage more aggressively precisely because the expected political return is higher (Pereira et al., 2020). We address this by using the federal authorization shock as an instrument for realized beneficiary expansion. The estimator is a cross-sectional IV regression in municipality-level changes between the 2008 and 2012 elections,

$$\Delta\text{Beneficiaries}_m = \pi_0 + \pi_1 \Delta\text{Quota}_m + \mathbf{X}_m^{0\prime}\boldsymbol{\pi}_2 + \eta_m \tag{3}$$

$$\Delta\text{VoteShare}_m = \beta_0 + \beta_1 \widehat{\Delta\text{Beneficiaries}}_m + \mathbf{X}_m^{0\prime}\boldsymbol{\beta}_2 + \varepsilon_m \tag{4}$$

where Δ denotes the 2012–2008 change and $\mathbf{X}_m^0 \in \mathbb{R}^K$ contains the pre-reform covariates used in each specification. The vectors $\boldsymbol{\pi}_2$ and $\boldsymbol{\beta}_2$ collect their first- and second-stage coefficients. The variable $\Delta\text{Beneficiaries}_m$ is the change in the twelve-month average of enrolled families between the 2008 and 2012 elections. The instrument ΔQuota_m is the cumulative quota shock defined in equation (2). The coefficient β_1 identifies the marginal effect of the municipality-level change in beneficiaries on the corresponding change in vote share, with the quota shock instrumenting the endogenous portion of beneficiary expansion.

This first-difference formulation makes the estimand transparent. The object of interest is the political return to beneficiary expansion between elections: how changes in realized enrollment from 2008 to 2012 translate into changes in incumbent vote share.

Both $\Delta\text{Beneficiaries}_m$ and ΔQuota_m are expressed per 10,000 residents using 2006 population. The tables report them in standard-deviation units, so the coefficients describe the response to a comparable change in each rate.

Equation (4) therefore compares two municipality-level rate changes: realized beneficiary expansion between elections, instrumented by the additional authorized slots opened during the term. One standard deviation of the quota shock is 171.50 authorized families per 10,000 residents, and one standard deviation of realized expansion is 181.36 beneficiary families per 10,000 residents.[9]

Treatment varies continuously rather than switching from zero to one. Following Angrist and Imbens (1995), β_1 averages the marginal electoral responses generated by

[9]This final standardization is a deterministic linear rescaling of the already-defined rates: it rescales coefficients and standard errors by the same constant, leaving t-statistics, p-values, first-stage strength, and Anderson–Rubin inference unchanged.

quota-induced enrollment changes. It should be read as the return to the expansion moved by the federal revisions, not as the effect of bringing every municipality from no program to full coverage.

Realized expansion is right-skewed, so municipalities with larger enrollment responses provide more of the identifying variation. Population weights give greater importance to municipalities where more people live. Because every municipality already had some program exposure, β_1 is a marginal response to further expansion rather than a comparison between having and not having *Bolsa Família*.[10]

Outcome. The main outcome is the change in the 2008 winner's vote share,

$$\Delta \text{VoteShare}_m = \frac{\text{IncumbentVotes}_{m,2012}}{\text{VotesOnCandidates}_{m,2012}} - \frac{\text{WinnerVotes}_{m,2008}}{\text{VotesOnCandidates}_{m,2008}}.$$

Here, $\text{VotesOnCandidates}_{mt}$ is the total number of valid votes cast for any candidate in municipality m in election year t, excluding blank and null ballots. The 2012 numerator refers to the same mayor elected in 2008. This normalization ensures that the outcome reflects the incumbent's competitive position among voters who expressed a preference for a candidate.

Sample. The estimation sample is restricted to municipalities in which the mayor elected in 2008 sought reelection in 2012. After excluding municipalities with missing treatment, outcome, or baseline covariate information and those without contested elections in both periods, the final sample contains 2,078 of the 5,492 municipalities in the complete selection universe, observed in two election years and yielding 4,156 municipality-year observations. This restriction follows from defining the outcome for the same mayor in both elections. Section 3.3.1 shows little relationship between the instrument and rerunning. Table 1 shows that the two samples are similar on baseline municipal characteristics, although the characteristics of the elected mayors differ.

Specification. All covariates enter as pre-reform levels in the first-difference regression. Column (1) includes indicators for the party of the incumbent elected in 2008 and baseline *Bolsa Família* intensity, measured as realized beneficiary families in 2008 relative to 2006 population. Column (2) adds predetermined municipality characteristics: log population in 2006 and Census 2000 urbanization, Gini, and adult illiteracy;

[10]The recent panel-DiD literature uses the same "ACR" label for related parameters identified under dose-level parallel-trends assumptions (notably Callaway et al., 2024). Our use follows the continuous-treatment IV framework of Angrist and Imbens (1995).

and characteristics of the incumbent mayor elected in 2008: gender, education, age and age squared, marital status, and indicators for public-sector, agricultural, and business occupations.

We do not include the incumbent's 2008 vote share in the preferred specification. The outcome is already the change in support for the same mayor, and conditioning again on its 2008 value would change the comparison and can introduce short-panel bias in a two-election design (Nickell, 1981; Imai and Kim, 2019).

All regressions use municipal population in 2006 as weights. The headline estimate therefore gives more importance to changes affecting larger populations. The appendix reports the full set of alternative weighting schemes, including equal municipal weights.

Standard errors allow municipalities in the same state to move together because the counterfactual formula uses a state-level poverty estimate (Abadie et al., 2023).[11]

3.3 Threats to Identification

3.3.1 Conditional Independence

Assumption 1 (Conditional Independence)**.** *Conditional on the incumbent party and the vector of baseline characteristics, the quota shock is orthogonal to the potential inter-election change in electoral outcomes:*

$$\begin{aligned}\mathbb{E}\big[\,\Delta Y_m(0) \;\big|\; \Delta\text{Quota}_m,\ \boldsymbol{\phi}_m^{\text{inc}},\ \mathbf{X}_m^0\,\big] \\ = \mathbb{E}\big[\,\Delta Y_m(0) \;\big|\; \boldsymbol{\phi}_m^{\text{inc}},\ \mathbf{X}_m^0\,\big].\end{aligned}$$

Here, $\Delta Y_m(0)$ is municipality m's counterfactual 2008–2012 change in incumbent vote share absent the quota shock. The quota shock must be unrelated to that change after accounting for incumbent party and pre-reform characteristics. This condition matters because the counterfactual quota uses a state-level poverty estimate, Poor_s^{2006}, and the 2012 quota revision incorporates updated federal poverty information. Municipalities in poorer states can receive different quota changes and also have different electoral dynamics (Zucco, 2013). The design therefore compares municipalities with similar pre-reform characteristics but different formula-driven quota shocks. The remaining identifying claim is that the two federal revisions were not targeted to the

[11]The regressions contain 26 state groups. We interpret borderline results cautiously, report wild-cluster checks for the main diagnostics, and use Anderson and Rubin (1949) confidence sets to show the uncertainty around IV estimates when the first stage is weak.

future electoral fortunes of particular mayors.

The proportional-selection exercise adapts Oster (2019) as a stability-style sensitivity check on the reduced-form coefficient of the quota shock. Oster's framework was developed for OLS on a single treatment, so we treat the resulting δ^* as a coefficient-stability heuristic rather than a structural bound: under instrument exogeneity the reduced form is causal on its own, and the proportional-selection assumption is invoked here only as a discipline on whether the observed coefficient is robust to the kind of selection that the observables already capture. Adding the predetermined municipality and mayor characteristics moves the coefficient on the quota shock from 1.93 to 1.30. Under Oster's recommended benchmark $\overline{R}^2 = 1.30 \cdot R^2_{\text{long}}$ we obtain $\delta^* = 2.79$; under the more demanding stress benchmark $\overline{R}^2 = 2.20 \cdot R^2_{\text{long}}$ we obtain $\delta^* = 0.70$. Thus, the recommended benchmark requires selection on unobservables 2.79 times as strong as selection on the included observables to overturn the reduced form, while the more aggressive $2.20 R^2_{\text{long}}$ benchmark lowers that threshold to 0.70.

Selection into rerunning. The estimation sample is restricted to municipalities where the 2008 incumbent sought reelection in 2012, so the instrument could bias inference if it systematically affected the decision to run again. Table 2 regresses an indicator for whether the 2008 incumbent appears on the 2012 ballot on the quota shock, estimated on the complete-case municipality universe before conditioning on rerunning. Under the preferred counterfactual-based instrument, the identifying variation appears orthogonal to selection into the estimation sample.

As a complementary check, Section 4.4 re-estimates the preferred specification weighted by the inverse probability of selection into the rerunning sample. The 2SLS point estimate moves from 4.69 to 4.14, which indicates that selection into the rerunning sample is not materially distorting the estimates.

Pre-reform electoral balance. A second exercise tests whether the quota shock is correlated with pre-existing political conditions. Table 3 reports placebo regressions of the 2008 winner's vote share on the preferred instrument within the estimation sample. Because the outcome was determined before the reform, any systematic non-zero association would signal baseline imbalance rather than the effect of quota-induced expansion.

The evidence is consistent with conditional orthogonality of the instrument. The point estimates are small, and their confidence intervals include zero as progressively

Table 2: Selection into Rerunning

	Incumbent Runs Again in 2012	
	(1)	(2)
ΔQuota_m	-2.183	0.045
	(1.156)	(1.017)
Incumbent Party	✓	✓
Bolsa Família Intensity	✓	✓
Municipality Characteristics		✓
Mayor Characteristics		✓
Number of Mayors / Municipalities	5,492	5,492
Observations	5,492	5,492

Notes: The dependent variable equals 100 if the mayor elected in 2008 appears on the 2012 ballot and zero otherwise. The estimation universe is constructed before conditioning on rerunning. Every column is weighted by 2006 population and includes state fixed effects because municipality and year effects are not identified in this one-observation-per-mayor design. The quota shock is a rate per 10,000 residents using fixed 2006 population and is divided by the main-sample analysis-weighted SD. Column (1) includes incumbent-party effects and baseline *Bolsa Família* intensity. Column (2) adds municipality and incumbent-mayor characteristics. These controls are baseline and pre-shock: party, program intensity, and mayor characteristics use 2008 values; population uses 2006; and the remaining municipality characteristics use the 2000 Census. Both columns therefore use only pre-shock controls. Standard errors clustered by state are reported in parentheses. A † attached to a standard error denotes a wild-cluster $p < 0.05$, with resampling by state.

Table 3: Pre-Reform Vote-Share Balance

	Incumbent's Vote Share in 2008	
	(1)	(2)
ΔQuota_m	0.127	-0.070
	(0.254)	(0.228)
Incumbent Party	✓	✓
Bolsa Família Intensity	✓	✓
Municipality Characteristics		✓
Mayor Characteristics		✓
Number of Mayors / Municipalities	2,078	2,078
Observations	2,078	2,078

Notes: The dependent variable is the vote share of the mayor elected in 2008, measured before the quota reform. Every column is weighted by 2006 population and includes state fixed effects. The quota shock is a rate per 10,000 residents using fixed 2006 population and is divided by its main-sample analysis-weighted SD. Column (1) includes incumbent-party effects and baseline *Bolsa Família* intensity. Column (2) adds municipality and incumbent-mayor characteristics. These controls are baseline and pre-shock: party, program intensity, and mayor characteristics use 2008 values; population uses 2006; and the remaining municipality characteristics use the 2000 Census. Both columns therefore use only pre-shock controls. Standard errors clustered by state are reported in parentheses. A † attached to a standard error denotes a wild-cluster $p < 0.05$, with resampling by state.

richer controls are added. We find no clear pre-reform imbalance in the 2008 winner's vote share.

No anticipation. The 2009 quota revision was formally announced in MDS *Informe* nº 169, dated April 2009, which communicated that approximately 1,300,000 additional families would be incorporated in staggered waves over the course of the year. The previous quota adjustment had occurred in 2006. Municipalities may have expected another update at some point, but the mayor elected in 2008 could not know the new formula, its municipal incidence, or its timing before taking office. The 2012 update was a second federal revision during the same term. The short-window specification removes that later update and isolates the initial 2009 change. The placebo results just above fit this timeline: the instrument has little relationship with the 2008 winner's vote share.

3.3.2 Exclusion Restriction

Assumption 2 (Exclusion Restriction)**.** *Let $Y_{m,t}(d, z)$ denote municipality m's potential incumbent vote share in election t under realized beneficiary enrollment d and quota shock z. For every m, t, d, and any two quota shocks z and z',*

$$Y_{m,t}(d, z) = Y_{m,t}(d, z') \equiv Y_{m,t}(d).$$

Thus, the quota shock can affect incumbent vote share only through its effect on realized beneficiary enrollment.

This is fundamentally a substantive rather than a statistical claim, and the case for it rests on the institutional channel through which the shock operates. The federal revisions changed the authorized number of beneficiary slots but did not alter the nationally determined benefit schedule, eligibility rules, or program conditions.

The institutional argument is the primary defense of the exclusion restriction, but the most plausible alternative channels have empirical counterparts that we can test directly. Two channels are worth addressing because they could, in principle, move with the same demographic inputs that enter the quota reform: the average per-family *Bolsa Família* benefit value and federal revenue-sharing transfers to municipalities (*Fundo de Participação dos Municípios*). The benefit schedule itself is set nationally and is not chosen by mayors, but the average benefit paid in a municipality can vary with family composition, since households with more children or adolescents receive additional variable benefits.

If the quota shock is correlated with local demographic composition, this could mechanically generate changes in average benefit values even holding enrollment fixed. *Fundo de Participação dos Municípios* (FPM) is also relevant because its transfers are assigned using population-based rules; if the demographic estimates used in the 2009 quota reform are correlated with the population brackets, the quota shock could proxy for concurrent fiscal transfers rather than only for beneficiary expansion. Institutionally, however, neither channel is part of the *Bolsa Família* quota formula: benefit values are fixed by federal decree, and FPM coefficients are set by Complementary Law 91/1989 from population brackets and apportionment factors.

Table 4 reports observable-channel diagnostics under the two pre-treatment control specifications. Panel A uses the change between the 12-month averages of total municipal FPM transfers in 2008 and 2012, per 10,000 residents using fixed 2006 population. Panel B uses the corresponding change in the simple 12-month average of the monthly per-family *Bolsa Família* benefit value. In Column (2), a one-standard-deviation quota shock is associated with 0.11 standard deviations of the FPM-transfer change and 0.08 standard deviations of the benefit-value change. Both reject zero under conventional state-clustered inference; FPM also rejects under the wild-cluster test, whereas benefit value does not. These regressions are observable-channel diagnostics, not tests that establish exclusion. The main specification instead conditions only on pre-reform FPM and benefit value, each interacted with post; the design's primary defense remains the institutional separation between the federal allocation reform and these alternative channels.

Reverse causality of realized expansion. Although it is conceptually distinct from the exclusion restriction, reverse causality between political strength and realized enrollment is the reason the design needs an instrument in the first place: if incumbents who were electorally stronger before the reform were also more able or willing to expand enrollment afterward, direct OLS estimates of the effect of realized expansion on vote share would be confounded.

The contrast between permitted and realized expansion makes the role of the instrument explicit. Baseline vote share does not predict formula-driven permitted expansion: the estimates remain close to zero across all three columns. It does predict realized expansion, however. The corresponding coefficients are -0.06, -0.05, and -0.04, and the wild-cluster test rejects zero in Columns (1) and (3). The negative sign does not identify a particular behavioral mechanism, but the non-zero association shows that realized enrollment reflects municipal choices and constraints correlated with prior elec-

Table 4: Alternative Exclusion Channels

	Alternative Exclusion Channels	
	(1)	(2)
Panel A: Δ Fundo de Participação dos Municípios (FPM) Transfers		
ΔQuota_m	0.082	0.107
	(0.058)	(0.020)†
Panel B: Δ Per-Family Bolsa Família Benefit Value		
ΔQuota_m	0.049	0.083
	(0.043)	(0.035)
Incumbent Party	✓	✓
Bolsa Família Intensity	✓	✓
Municipality Characteristics		✓
Mayor Characteristics		✓
Number of Mayors / Municipalities	2,078	2,078
Observations	4,156	4,156

Notes: Panel A uses the change between the 12-month average of total municipal FPM transfers in 2008 and 2012, per 10,000 residents using fixed 2006 population, divided by its full-sample 2006-population-weighted SD. Panel B uses the change between the simple averages of the twelve monthly per-family benefit values in 2008 and 2012, divided by the cross-municipality SD of its change. The analysis-weighted mean changes are R$57,959.64 per month per 10,000 residents for FPM and R$49.48 per family per month for benefit value. Multiplying the standardized Column (2) quota coefficients by the corresponding outcome SD gives R$5,466.59 per month per 10,000 residents and R$0.91 per family per month, respectively. All models use 2006-population weights. Each displayed row is a reduced-form channel diagnostic and is not conditioned on realized beneficiary expansion. Column (1) includes incumbent-party effects and baseline *Bolsa Família* intensity. Column (2) adds municipality and incumbent-mayor characteristics. These controls are baseline and pre-shock: party, program intensity, and mayor characteristics use 2008 values; population uses 2006; and the remaining municipality characteristics use the 2000 Census. Both columns therefore use only pre-shock controls. Standard errors clustered by state are reported in parentheses. A † attached to a standard error denotes a wild-cluster $p < 0.05$, with resampling by state.

toral performance. Direct OLS comparisons of realized growth and subsequent votes would therefore confound the political return to expansion with the process determining which municipalities expand. The federal quota shock breaks this link by isolating authorized expansion generated by the national allocation rule rather than by municipal political incentives, which is precisely why realized enrollment is instrumented.

3.3.3 Monotonicity

Assumption 3 (Weak Monotonicity)**.** *For every municipality m, a positive quota shock cannot reduce realized expansion:* $\Delta\text{Beneficiaries}_m(z') \geq \Delta\text{Beneficiaries}_m(z)$ *whenever* $z' > z$*, where* $\Delta\text{Beneficiaries}_m(z)$ *is the potential inter-election change in beneficiaries that municipality m would realize under shock value z.*

As in any setting, this is an identifying restriction rather than a testable hypothesis,

Table 5: Reverse Causality Channels

	Reverse Causality Channels		
	(1)	(2)	(3)
Panel A: Δ Permitted Bolsa Família Expansion			
Incumbent's Vote Share in 2008	0.009	-0.005	-0.008
	(0.018)	(0.016)	(0.019)
Panel B: Δ Realized Bolsa Família Expansion			
Incumbent's Vote Share in 2008	-0.058	-0.049	-0.042
	(0.026)†	(0.022)	(0.019)†
Incumbent Party	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓
Municipality Characteristics		✓	✓
Mayor Characteristics		✓	✓
Benefit Value			✓
FPM Transfers			✓
Number of Mayors / Municipalities	2,078	2,078	2,078
Observations	2,078	2,078	2,078

Notes: Panel A measures permitted quota expansion in quota-shock-SD units. Panel B measures the 2008–2012 net change in the 12-month average beneficiary-family stock in treatment-change-SD units. The regressor is the 2008 winner's vote share divided by its analysis-weighted SD of 9.59 percentage points. Every column is weighted by 2006 population and includes state fixed effects. Columns (1)–(2) follow the pre-treatment control sequence. Column (3) additionally includes 2008 benefit value and FPM transfers. Standard errors clustered by state are reported in parentheses. A † attached to a standard error denotes a wild-cluster $p < 0.05$, with resampling by state.

since we never observe both potential outcomes for the same unit. We follow standard practice in the continuous-treatment IV literature (Imbens and Angrist, 1994; Angrist and Imbens, 1995) by combining an institutional argument with empirical diagnostics consistent with weak monotonicity. These diagnostics probe the shape of the average first stage, which is observable, but cannot validate municipality-level monotonicity, which concerns unobserved potential treatments.

The institutional argument is direct. The quota shock changes the amount of authorized room a municipality has to add beneficiaries; it does not mandate reductions in existing caseloads. Even municipalities with negative quota adjustments were not required, or legally allowed, to remove current beneficiaries simply because their authorized ceiling fell. Beneficiaries could be cancelled only through the program's regular eligibility and irregularity checks. A lower quota therefore restricts future incorporation: newly registered eligible families are less likely to be added to the payroll where authorized room is scarce.

By contrast, municipalities receiving positive quota shocks acquire additional slots that can be converted into realized enrollment if local officials identify and register eligible households. The monotonicity claim is therefore about the direction of the opportunity set: more authorized room can relax the enrollment constraint, while less authorized room does not mechanically force beneficiary removal. A municipality that received a positive shock acquired headroom to enroll additional families if it chose to, while one that received a smaller adjustment lost no pre-existing capacity. There is no mechanical channel through which a higher federal authorization depresses enrollment. Defier behavior is possible in principle, since a mayor could scale back registration just as the ceiling expanded, but we view that response as institutionally implausible.

Table 6 reports a transparent average-shape diagnostic. Each panel estimates separate quota-shock slopes for the named upper-tail group and its complement on the full locked sample, and reports the upper-tail coefficient. The groups are the nested top 50, top 25, top 15, top 5, and top 1 percent of the quota-shock distribution; the dependent variable is always the standardized change in realized beneficiaries, shown in the columns, and the regressor is the standardized quota change, shown in the row. Under the complete-control specification, the five upper-tail slopes are 0.53, 0.53, 0.49, 0.63, and 0.83. All are positive. The wild-cluster results support a positive slope through the top 5 percent; the top-1-percent estimate remains positive but is based on a much smaller cell and is correspondingly less precise.

This pattern is consistent with the first-stage response strengthening rather than reversing among municipalities receiving the largest authorization shocks. It should not be read as a formal test of monotonicity: the nested groups overlap, their slope coefficients need not themselves rise at every cutoff, and the diagnostic concerns average observed responses rather than municipality-level potential treatments. Its evidentiary role is narrower: alongside the institutional fact that additional authorization expands rather than contracts the feasible enrollment set, the table provides no indication of an average first-stage reversal in the region of the support carrying the largest shocks.

3.3.4 The 2012 Quota Revision

Municipal authorization margins were revised again in 2012, shortly before the election.[12] We incorporate this revision deliberately because the estimand covers the mayor's entire term. Municipal governments could register eligible households in

[12]Following the release of the 2010 Census, the federal government updated municipal poverty estimates used to guide program expansion in 2012. See the MDS *Informe* nº 324.

Table 6: Monotonicity by Quota-Shock Size

	ΔBeneficiaries$_m$		
	(1)	(2)	(3)
Panel A: Top 50%			
ΔQuota$_m$	0.504	0.545	0.527
	(0.062)†	(0.057)†	(0.056)†
Panel B: Top 25%			
ΔQuota$_m$	0.485	0.518	0.531
	(0.091)†	(0.071)†	(0.066)†
Panel C: Top 15%			
ΔQuota$_m$	0.331	0.440	0.494
	(0.153)	(0.116)†	(0.099)†
Panel D: Top 5%			
ΔQuota$_m$	0.510	0.604	0.630
	(0.181)	(0.124)†	(0.104)†
Panel E: Top 1%			
ΔQuota$_m$	0.776	0.801	0.828
	(0.163)	(0.135)	(0.128)†
Incumbent Party	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓
Municipality Characteristics		✓	✓
Mayor Characteristics		✓	✓
Benefit Value			✓
FPM Transfers			✓

Notes: Each panel estimates separate slopes for the named upper-tail group and its complement on the full locked sample; the table reports the upper-tail slope. Groups are defined by unweighted quota-shock quantiles and are nested. Columns follow the headline control sequence, including baseline benefit value and FPM transfers in Column (3), with 2006-population weights and state-clustered standard errors. † denotes the wild-cluster result. This is an average first-stage-shape diagnostic, not a test of unit-level monotonicity.

the *Cadastro Único* throughout 2009–2012. When new federal slots were released in 2012, eligible families already in the registry but not yet receiving *Bolsa Família* could be incorporated automatically before the election. Excluding the 2012 authorization would therefore discard part of the return to municipal registration effort undertaken during the same term.

The headline instrument accordingly measures official 2012 authorization relative to the R$90/137 counterfactual 2009 quota, while the endogenous variable measures realized enrollment growth from the twelve-month average in 2008 to the twelve-month average in 2012. The design captures cumulative expansion relative to the allocation implied by the previous rule; it does not interpret the 2012 revision as a separate treatment. Table 10 reports a Short Window check that pairs the 2009 quota shock with the change between the twelve-month averages for 2008 and 2009, isolating the initial reform horizon.

4 Main Results

We proceed in four steps. First, we establish that the quota changes generated substantial variation in realized program expansion. Second, we estimate the reduced-form relationship between federal authorization and electoral performance. Third, we combine these two moments to estimate the response of incumbent vote share and turnout to instrumented beneficiary expansion. Fourth, we examine robustness and heterogeneity. The treatment and instrument are shown in standard-deviation units, so the coefficients refer to a comparable change in each variable.

4.1 First Stage: Authorization and Coverage

Table 7 reports the first-stage relationship between the quota shock and realized beneficiary expansion. In the preferred specification, a one-standard-deviation increase in the quota shock raises standardized beneficiary enrollment by 0.30 standard deviations. The first-stage coefficient is 0.32 in Column (1), 0.36 in Column (2), and 0.30 in Column (3). The stability across columns shows that the relationship is not driven by the addition of the municipality, mayor, benefit-value, or FPM-transfer controls.

The first-stage magnitude is economically substantial. One standard deviation of the shock corresponds to 171.50 authorized beneficiary families per 10,000 residents, and the preferred coefficient implies a realized response of 54.68 beneficiary families

Table 7: First-Stage Estimates

	ΔBeneficiaries$_m$		
	(1)	(2)	(3)
ΔQuota$_m$ (π_1)	0.317	0.359	0.301
	(0.063)	(0.062)	(0.052)
Kleibergen–Paap F	25.43	33.63	33.30
Incumbent Party	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓
Municipality Characteristics		✓	✓
Mayor Characteristics		✓	✓
Benefit Value			✓
FPM Transfers			✓
Number of Mayors	2,078	2,078	2,078
Observations	4,156	4,156	4,156

Notes: All columns use 2006-population weights and state-clustered standard errors. The instrument and endogenous variable are rates per 10,000 residents using fixed 2006 population and are reported per one analysis-weighted SD. Column (1) includes incumbent-party and baseline program-intensity controls; Column (2) adds municipality and mayor characteristics; Column (3) adds 2008 benefit value and FPM transfers, each interacted with post. Standard errors are in parentheses.

per 10,000 residents. In natural units the first-stage slope is therefore 0.32 realized families per additional authorized beneficiary in the recentered shock. This is a projection coefficient, not a take-up rate, and the 12-month treatment contrast measures the net change in the average monthly beneficiary-family stock rather than gross program entries.

4.2 Reduced Form: Authorization and Electoral Returns

Table 8 reports the relationship between the inter-election quota shock, measured as the official 2012 authorization relative to the R$90/137 counterfactual 2009 quota, and incumbent electoral performance. This is the most direct test of whether additional federal authorization translates into electoral gains. Its interpretation as an authorization effect relies on the quota shock being unrelated to the mayor's underlying electoral trend, but it does not require the further claim that authorization affects votes only through realized enrollment.

In the preferred specification, a one-standard-deviation increase in the quota shock raises the incumbent's vote share by 1.41 percentage points. The estimate moves from 1.93 in Column (1) to 1.30 in Column (2) and 1.41 in Column (3). The turnout estimates

Table 8: Reduced-Form Estimates

	Δ Incumbent's Vote Share			Δ Turnout		
	(1)	(2)	(3)	(4)	(5)	(6)
ΔQuota_m (ρ_1)	1.929	1.299	1.414	-0.372	-0.352	-0.354
	(0.604)	(0.461)	(0.482)	(0.192)	(0.181)	(0.180)
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics		✓	✓		✓	✓
Mayor Characteristics		✓	✓		✓	✓
Benefit Value			✓			✓
FPM Transfers			✓			✓
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: All columns use 2006-population weights and state-clustered standard errors. The instrument and endogenous variable are rates per 10,000 residents using fixed 2006 population and are reported per one analysis-weighted SD. Column (1) includes incumbent-party and baseline program-intensity controls; Column (2) adds municipality and mayor characteristics; Column (3) adds 2008 benefit value and FPM transfers, each interacted with post. Standard errors are in parentheses.

in Columns (4)–(6) are small and do not clearly differ from zero.

4.3 Second Stage: Coverage and Electoral Returns

Table 9 reports two-stage least squares estimates of the effect of instrumented beneficiary expansion on incumbent electoral performance. Under the first-difference specification in equation (4), the coefficient identifies the marginal effect of the municipality-level inter-election change in beneficiaries on the corresponding change in vote share.

In the preferred specification, a one-standard-deviation increase in instrumented beneficiary expansion raises the incumbent's vote share by 4.69 percentage points. The vote-share confidence set excludes zero, while the turnout estimate is imprecise and its confidence set includes zero.

The substantive meaning is easier to see in natural units. One standard deviation of the 2008–2012 net enrollment change is 181.36 beneficiary families per 10,000 residents. The preferred estimate therefore implies that an increase of 100 beneficiary families per 10,000 residents raises incumbent vote share by 2.59 percentage points. This is a rescaling of the same 2SLS coefficient, not a different estimate.

The preferred estimate of 4.69 percentage points is meaningful relative to the stakes of a municipal race. The median inter-election decline in incumbent support in our sam-

Table 9: Two-Stage Least Squares Estimates

	Δ Incumbent's Vote Share			Δ Turnout		
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	6.082	3.619	4.690	-1.173	-0.981	-1.173
	(2.322)	(1.569)	(1.991)	(0.698)	(0.532)	(0.639)
AR 95% CI	[2.019, 12.834]	[0.876, 7.978]	[1.242, 10.282]	[-3.168, 0.069]	[-2.302, 0.060]	[-2.792, 0.053]
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics		✓	✓		✓	✓
Mayor Characteristics		✓	✓		✓	✓
Benefit Value			✓			✓
FPM Transfers			✓			✓
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: All columns use 2006-population weights and state-clustered standard errors. The instrument and endogenous variable are rates per 10,000 residents using fixed 2006 population and are reported per one analysis-weighted SD. Column (1) includes incumbent-party and baseline program-intensity controls; Column (2) adds municipality and mayor characteristics; Column (3) adds 2008 benefit value and FPM transfers, each interacted with post. Standard errors are in parentheses. The Kleibergen–Paap statistic is the squared state-clustered first-stage t-ratio. Anderson–Rubin 95% confidence sets use quadratic state-clustered inversion with a t_{G-1} critical value and are robust to arbitrary instrument weakness.

ple is 3.03 percentage points, of the same order as the incumbency disadvantage that Klašnja and Titiunik (2017) document for Brazilian municipal elections. This comparison gives the estimate scale; it does not mean that every incumbent receives a three-point gain. In separate subsamples defined by close 2008 races, the vote-share estimates are close to zero and imprecise. The evidence therefore supports a meaningful average response without showing that it is concentrated in the closest elections.

4.4 Robustness

Table 10 presents the core sensitivity analysis for vote share across seven specifications. Column (1) reproduces the preferred estimate, Columns (2) and (3) use alternative outcomes, Columns (4) and (5) use alternative quota shocks, Column (6) adjusts for selection into the rerunning sample, and Column (7) normalizes the treatment and instrument by Census-2000 families.

Table 10: Robustness Checks: Vote Share

	Δ Incumbent's Vote Share						
		Alt. Outcomes		Alt. Shock		Alt. Weights	Alt. Scale
	Main	Winsorized	Registered Votes	Winsorized	Limited to 2009	IPW	Per Families
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: First Stage							
ΔQuota_m (π_1)	0.301	0.301	0.301	0.277	0.236	0.302	0.281
	(0.052)	(0.052)	(0.052)	(0.043)	(0.034)	(0.049)	(0.048)
Kleibergen–Paap F	33.30	33.30	33.30	41.26	48.07	37.68	34.88
Panel B: Reduced Form							
ΔQuota_m (ρ_1)	1.414	1.277	1.145	1.523	0.584	1.250	1.295
	(0.482)	(0.429)	(0.407)	(0.478)	(0.621)	(0.452)	(0.507)
Panel C: Two-Stage Least Squares							
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	4.690	4.234	3.797	5.500	2.474	4.144	4.604
	(1.991)	(1.773)	(1.654)	(2.082)	(2.570)	(1.758)	(2.236)
AR 95% CI	[1.242, 10.282]	[1.164, 9.209]	[0.914, 8.408]	[1.782, 11.012]	[-3.245, 7.846]	[0.978, 8.809]	[0.772, 10.906]
Incumbent Party	✓	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓	✓
Municipality Characteristics	✓	✓	✓	✓	✓	✓	✓
Mayor Characteristics	✓	✓	✓	✓	✓	✓	✓
Benefit Value	✓	✓	✓	✓	✓	✓	✓
FPM Transfers	✓	✓	✓	✓	✓	✓	✓
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156	4,156

Notes: Column (1) is the preferred valid-vote specification. Columns (2)–(3) winsorize the outcome at the 1st and 99th percentiles or divide incumbent votes by registered voters. Columns (4)–(5) winsorize the quota shock or replace the 2012 authorization and 2008–2012 beneficiary change with the corresponding 2009 authorization and 2008–2009 beneficiary change. Column (6) combines inverse-selection and population weights. Column (7) expresses the instrument and treatment per 100 Census-2000 families. Every column includes the complete control set and state-clustered standard errors. AR sets use cluster-robust quadratic inversion.

Table 11: Continuous-Quota and Discrete-Beneficiary Specifications

	Δ Incumbent's Vote Share			Δ Turnout		
	(1)	(2)	(3)	(4)	(5)	(6)
Reduced Form (*common to all panels*)						
ΔQuota_m (ρ_1)	1.929	1.299	1.414	-0.372	-0.352	-0.354
	(0.604)	(0.461)	(0.482)	(0.192)	(0.181)	(0.180)
Panel A: Continuous						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	6.082	3.619	4.690	-1.173	-0.981	-1.173
	(2.322)	(1.569)	(1.991)	(0.698)	(0.532)	(0.639)
AR 95% CI	[2.019, 12.834]	[0.876, 7.978]	[1.242, 10.282]	[-3.168, 0.069]	[-2.302, 0.060]	[-2.792, 0.053]
Panel B: Above Median						
$\mathbf{1}\{\Delta\widehat{\text{Beneficiaries}}_m \geq c_D\}$ (β_1)	30.129	15.579	21.951	-5.812	-4.223	-5.488
	(14.379)	(7.838)	(11.340)	(3.641)	(1.953)	(2.558)
AR 95% CI	[9.445, 123.527]	[3.430, 44.533]	[5.090, 67.370]	[-25.814, 0.379]	[-9.013, 0.335]	[-12.189, 0.328]
Panel C: Top 25 Percent						
$\mathbf{1}\{\Delta\widehat{\text{Beneficiaries}}_m \geq c_D\}$ (β_1)	24.045	13.966	17.647	-4.638	-3.786	-4.412
	(8.792)	(5.342)	(6.303)	(2.742)	(2.129)	(2.380)
AR 95% CI	[8.279, 49.109]	[3.826, 28.051]	[5.440, 33.661]	[-12.464, 0.273]	[-9.444, 0.225]	[-10.448, 0.204]
Panel D: Above the 90th Percentile						
$\mathbf{1}\{\Delta\widehat{\text{Beneficiaries}}_m \geq c_D\}$ (β_1)	29.649	18.877	22.446	-5.719	-5.117	-5.612
	(12.181)	(7.744)	(8.693)	(3.565)	(3.176)	(3.431)
AR 95% CI	[9.474, 69.956]	[4.972, 41.702]	[6.668, 47.813]	[-17.252, 0.322]	[-15.024, 0.273]	[-16.290, 0.228]
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics		✓	✓		✓	✓
Mayor Characteristics		✓	✓		✓	✓
Benefit Value			✓			✓
FPM Transfers			✓			✓
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: The standardized quota shock remains continuous in every panel. Panel A also retains the continuous beneficiary change. Panels B–D instead define the endogenous variable as an indicator that the municipality's 2008–2012 beneficiary change is at or above the sample median, 75th percentile, or 90th percentile, respectively. The reduced form is identical across panels by construction and is shown once above them. The 2SLS coefficient in Panels B–D is the effect of crossing the displayed beneficiary-expansion threshold. Each column follows the headline control sequence. Weights, clustering, and AR inversion otherwise match the main tables.

The discretization exercise changes the endogenous variable while preserving the continuous quota shock that identifies the reduced form. The preferred reduced-form estimate is therefore 1.41 percentage points in every panel by construction. Panel A retains the continuous beneficiary change and reproduces the headline 2SLS estimate of 4.69. Panels B–D instead ask whether quota-induced expansion moves a municipality above progressively more demanding thresholds of realized beneficiary growth.

The corresponding 2SLS estimates are 21.95 for moving above the sample median,

17.65 for entering the top quartile, and 22.45 for moving above the 90th percentile. These coefficients summarize quota-induced movement across each expansion threshold and therefore are not measured in the same units as the continuous estimate in Panel A. The exercise preserves the ordering of the treatment margin while allowing the electoral response to be summarized without imposing linearity in realized expansion.

The vote-share estimates are 4.23 under outcome winsorization and 3.80 when registered voters form the denominator. Winsorizing the quota shock yields 5.50. The short window, which pairs the 2009 quota change with the change between the twelve-month enrollment averages for 2008 and 2009, yields 2.47. The selection adjustment yields 4.14, and normalization by Census-2000 families yields 4.60.

4.5 Heterogeneity

We close the main results with four paired sample partitions and a separate municipality-size exercise. In each paired partition, the instrument and the endogenous variable are interacted with a binary indicator and its complement, yielding two coefficients per equation from a single full-sample regression. The decomposition tests the augmented model's central prediction that the first stage attenuates on partitions aligned with the structural margins the model identifies, and remains comparable on partitions orthogonal to them.

The Northeast partition (Columns 1–3) leaves the first stage strong on both sides. The 2SLS estimates are 2.71 outside the Northeast and 1.01 within it; their displayed difference, 1.70, is imprecise. Region therefore does not generate a statistically clear contrast in the per-beneficiary return.

The PT partition (Columns 4–6) yields a 2SLS estimate of 6.49 for incumbents from other parties and -1.37 for PT incumbents. Only the other-party Anderson–Rubin set excludes zero, and the displayed difference of 7.86 is not statistically distinguishable from zero.

The Frey partition (Columns 1–3) is sharper. The 2SLS estimate is 14.20 outside the cell that anchors Frey (2019)'s identification and -0.12 within it, for a displayed difference of 14.32. The outside-Frey first stage is comparatively weak, so the wide Anderson–Rubin set is the appropriate guide to the uncertainty around that large point estimate. Columns (4)–(6) report the corresponding below- and above-median split in baseline *Bolsa Família* intensity.

Table 12: Heterogeneity by Subgroup: Northeast States and PT Incumbent

	Northeast States			PT Incumbent		
	Other Regions	Northeast	Δ	Other Parties	PT	Δ
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.390	0.495	-0.105	0.280	0.365	-0.085
	(0.075)	(0.031)	(0.082)	(0.036)	(0.125)	(0.109)
Conditional First-Stage F	26.85	247.95		70.40	9.58	
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	1.059	0.502	0.556	1.814	-0.501	2.315
	(0.871)	(0.409)	(0.962)	(0.630)	(0.982)	(1.447)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	2.712	1.015	1.697	6.490	-1.374	7.864
	(2.396)	(0.801)	(2.526)	(2.716)	(2.678)	(4.838)
Conditional AR 95% CI	[-1.789, 9.135]	[-0.727, 2.604]		[1.666, 13.098]	[-10.589, 13.499]	
Incumbent Party	✓	✓		✓	✓	
Bolsa Família Intensity	✓	✓		✓	✓	
Municipality Characteristics	✓	✓		✓	✓	
Mayor Characteristics	✓	✓		✓	✓	
Benefit Value	✓	✓		✓	✓	
FPM Transfers	✓	✓		✓	✓	
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: Each three-column block uses the full headline sample. Each block is one fully interacted dual-IV regression. The instrument, endogenous variable, and complete control set are allowed to vary across the complementary subgroup indicators; Δ is the first coefficient minus the second. The saturated system reproduces separate subgroup coefficients while retaining one joint model for the displayed comparison. All models use 2006-population weights and state-clustered standard errors. Contrast columns are calculated from the joint model and therefore do not receive control check marks. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). Group AR sets are plug-in conditional diagnostics that condition on the other group coefficient, not exact joint weak-IV regions.

Table 13: Heterogeneity by Subgroup: Frey Sample and Baseline *Bolsa Família* Intensity

	Frey 2019			Baseline *Bolsa Família* Intensity		
	Outside Sample	Inside Sample	Δ	Below Median	Above Median	Δ
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.213	0.366	-0.153	0.255	0.370	-0.115
	(0.068)	(0.055)	(0.064)	(0.044)	(0.055)	(0.057)
Conditional First-Stage F	9.75	44.76		33.42	45.09	
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	3.017	-0.045	3.062	2.603	-0.150	2.753
	(0.962)	(0.270)	(0.994)	(1.056)	(0.222)	(1.162)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	14.198	-0.122	14.320	10.192	-0.405	10.598
	(7.255)	(0.736)	(7.213)	(4.301)	(0.612)	(4.599)
Conditional AR 95% CI	[3.867, 52.461]	[-1.706, 1.483]		[1.723, 20.805]	[-1.841, 0.815]	
Incumbent Party	✓	✓		✓	✓	
Bolsa Família Intensity	✓	✓		✓	✓	
Municipality Characteristics	✓	✓		✓	✓	
Mayor Characteristics	✓	✓		✓	✓	
Benefit Value	✓	✓		✓	✓	
FPM Transfers	✓	✓		✓	✓	
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: Each three-column block uses the full headline sample. The Frey sample comprises municipalities with 2000 population below 30,000 and 2010 HDI below 0.7. Each block is one fully interacted dual-IV regression. The instrument, endogenous variable, and complete control set are allowed to vary across the complementary subgroup indicators; Δ is the first coefficient minus the second. The saturated system reproduces separate subgroup coefficients while retaining one joint model for the displayed comparison. All models use 2006-population weights and state-clustered standard errors. Contrast columns are calculated from the joint model and therefore do not receive control check marks. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). Group AR sets are plug-in conditional diagnostics that condition on the other group coefficient, not exact joint weak-IV regions.

We are careful, however, about why that margin is missing. The saturation measures are built from 2008 data, before our 2009 shock but after the earlier, federally driven expansion that Frey (2019) studies; a high 2008 utilization rate in the Frey cell may therefore be a product of that prior expansion rather than a fixed structural ceiling. We read the contrast accordingly: not that these municipalities are intrinsically unable to respond, but that the federally controlled enrollment channel had already absorbed much of the eligible pool here, leaving no room for the 2009 authorization to operate. Because the partition is dated 2008, it is predetermined with respect to our shock and does not threaten identification; it simply locates our variation in a different region of the support. This sharpens the distinction between the two designs: Frey (2019) identifies the electoral return to a federally controlled enrollment channel in the regime where it still had room to grow, whereas we identify the return to a locally controlled channel where room remains.

The partisan split therefore provides no affirmative evidence that the per-beneficiary return is larger under PT mayors. The first stages are similar in sign and the coefficient difference is too imprecise to carry independent weight.

The institutional basis for a possible partisan-attribution channel remains clear: *Bolsa Família* was created under the federal Lula administration, and both the 2009 reform and the 2012 municipal election occurred under continuous PT control of the federal executive. The estimates, however, do not support a claim that this common partisan chain raises the local return. The paper's central result is the pooled electoral response, not a party-specific differential.

Municipality size was an ex ante concern because the headline specification gives more importance to places where more people live. Table 14 shows no clear electoral response below the median or in the broader sample below the 75th percentile. Above the 75th percentile, the reduced form is 2.91 and the 2SLS estimate is 11.27; above the 90th percentile they rise to 5.42 and 18.26. The headline response therefore comes mainly from larger municipalities. This is a statement about where the result appears, not a separate effect of municipality size.

5 Possible Mechanisms

The main results establish that federally induced quota expansion generated electoral returns for incumbent mayors. This section uses the same framework to examine the model's predictions about which margins of capacity must be in place for the federal-

Table 14: Heterogeneity by Municipality Size

	Δ Incumbent's Vote Share			Δ Turnout		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Below Median						
ΔQuota_m (ρ_1)	-0.147	-0.057	0.033	-0.167	-0.172	-0.223
	(0.318)	(0.289)	(0.292)	(0.117)	(0.103)	(0.109)
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-0.303	-0.114	0.073	-0.345	-0.347	-0.493
	(0.675)	(0.585)	(0.643)	(0.260)	(0.216)	(0.265)
AR 95% CI	[-1.979, 0.961]	[-1.473, 1.040]	[-1.435, 1.369]	[-0.985, 0.145]	[-0.851, 0.078]	[-1.164, 0.003]
Panel B: Below the 75th Percentile						
ΔQuota_m (ρ_1)	-0.078	-0.138	0.042	-0.290	-0.230	-0.291
	(0.315)	(0.329)	(0.291)	(0.198)	(0.159)	(0.156)
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-0.200	-0.312	0.109	-0.740	-0.519	-0.755
	(0.800)	(0.740)	(0.756)	(0.571)	(0.384)	(0.448)
AR 95% CI	[-1.844, 1.662]	[-1.881, 1.275]	[-1.468, 1.790]	[-2.288, 0.267]	[-1.442, 0.208]	[-1.879, 0.075]
Panel C: Above the 75th Percentile						
ΔQuota_m (ρ_1)	3.873	2.584	2.908	-0.412	-0.487	-0.430
	(1.113)	(0.883)	(0.928)	(0.203)	(0.246)	(0.278)
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	12.596	7.731	11.270	-1.341	-1.458	-1.666
	(4.061)	(3.173)	(4.840)	(0.775)	(0.772)	(1.158)
AR 95% CI	[5.274, 24.648]	[2.195, 17.645]	[3.368, 27.324]	[-3.703, 0.018]	[-3.592, 0.065]	[-4.931, 0.558]
Panel D: Above the 90th Percentile						
ΔQuota_m (ρ_1)	6.790	5.301	5.424	-0.346	-0.537	-0.448
	(1.425)	(0.947)	(1.048)	(0.204)	(0.312)	(0.345)
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	20.717	14.167	18.257	-1.056	-1.434	-1.509
	(7.342)	(4.990)	(7.058)	(0.824)	(0.989)	(1.383)
AR 95% CI	[10.647, 71.349]	[7.342, 42.878]	[8.677, 60.479]	[-6.167, 0.200]	[-5.886, 0.294]	[-8.196, 0.813]
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics		✓	✓		✓	✓
Mayor Characteristics		✓	✓		✓	✓
Benefit Value			✓			✓
FPM Transfers			✓			✓

Notes: Panels use the 2006-population median, 75th percentile, and 90th percentile calculated in the headline sample. The below-median and below-75th panels are nested; the above-90th-percentile panel is a subset of the above-75th panel. Models are estimated separately within each displayed sample. Columns follow the headline control sequence; all other specification choices match the main tables. No mean column is reported.

to-local chain to operate. We organize the evidence around two margins: infrastructure (κ^I) and alignment (κ^A). These comparisons describe the conditions under which the authorization shock produces enrollment and electoral returns.

Our empirical approach follows the IV literature on differences in the first stage across groups. Municipalities may respond differently depending on their initial conditions, so we let the instrument, treatment, and controls vary with each group indicator. This gives the same coefficient estimates as separate regressions while preserving a direct comparison between groups. The approach follows Angrist and Imbens (1995) and the recent discussion of saturation and first-stage heterogeneity in Blandhol et al. (2022) and Abadie et al. (2024). Because each partition contains more than one instrumented treatment, we report the conditional first-stage F proposed by Sanderson and Windmeijer (2016) for each group, together with the AR intervals used throughout the paper.

5.1 Infrastructure: κ^I

Of the 2,078 municipalities in the estimation sample, 1,394 (67.08 percent) had at least one *CRAS* in the 2008 *Censo SUAS*; the remaining 684 did not. The decomposition that follows asks whether the same quota shock produces different enrollment and electoral responses across these predetermined capacity states.

The first stage is strong in both cells: 0.30 in municipalities with a *CRAS* and 0.32 without one. The asymmetry instead appears in the electoral reduced form, which is 1.84 with a *CRAS*; without one, the confidence interval includes zero. Thus preexisting infrastructure does not determine whether authorization becomes enrollment. It conditions where that expansion produces a clear incumbent vote-share return. This pattern is consistent with the broader finding that municipal implementation shapes distributive politics (Fried, 2012; Sugiyama and Hunter, 2013).

The 2SLS coefficients sharpen this electoral asymmetry. The vote-share estimate is 6.11 in the *CRAS* cell, while the estimate outside that cell is not distinguishable from zero. Because *CRAS* presence is measured before the reform, we read this as a predetermined scope condition for the implementation channel.

The operational partitions provide a finer version of the same comparison. Table 16 distinguishes *CRAS* municipalities by active outreach, and Table 17 distinguishes them by whether any *Bolsa Família* families were tracked through PAIF. These indicators are measured before the quota shock and are interpreted as scope conditions.

Table 15: Heterogeneity by CRAS Presence

	Δ Incumbent's Vote Share			Δ Turnout		
	Without CRAS	With CRAS	Δ	Without CRAS	With CRAS	Δ
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.325	0.301	0.023	0.325	0.301	0.023
	(0.040)	(0.060)	(0.056)	(0.040)	(0.060)	(0.056)
Conditional First-Stage F	66.76	25.05		66.76	25.05	
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	-0.398	1.841	-2.239	-0.138	-0.385	0.247
	(0.634)	(0.540)	(0.736)	(0.073)	(0.213)	(0.206)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-1.226	6.111	-7.338	-0.425	-1.277	0.852
	(1.935)	(2.394)	(2.518)	(0.220)	(0.765)	(0.733)
Conditional AR 95% CI	[-5.247, 2.991]	[2.123, 13.449]		[-0.897, 0.039]	[-3.332, 0.176]	
Incumbent Party	✓	✓		✓	✓	
Bolsa Família Intensity	✓	✓		✓	✓	
Municipality Characteristics	✓	✓		✓	✓	
Mayor Characteristics	✓	✓		✓	✓	
Benefit Value	✓	✓		✓	✓	
FPM Transfers	✓	✓		✓	✓	
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: Each outcome is estimated in one fully interacted dual-IV system allowing the standardized instrument, treatment, and complete control set to vary with 2008 *CRAS* presence and its complement; Δ is the first coefficient minus the second. The saturated system reproduces separate subgroup coefficients while retaining one joint model for the displayed comparison. Models use the headline sample, 2006-population weights, and state-clustered standard errors. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). AR sets are plug-in conditional diagnostics, not exact joint weak-IV regions.

5.2 Alignment: κ^A

Conditional on the existence of this delivery apparatus, the next question is whether the apparatus is politically usable by the incoming mayor. The augmented model predicts that the alignment margin κ^A is the second binding constraint: the group-specific first stage should be weaker in *CRAS* municipalities where we do not observe the incoming mayor replacing the front-line manager on taking office.

Table 19 reports the corresponding within-*CRAS* decomposition, comparing municipalities with an observed manager change to the remaining *CRAS* municipalities. We define the manager as the *de facto* frontline operator: the respondent to the unit-level *Censo SUAS* questionnaire, that is, the person who fills out the form and runs the unit

Table 16: Heterogeneity by CRAS Intensity: Active Outreach

	Δ Incumbent's Vote Share			Δ Turnout		
	Without Active Outreach	With Active Outreach	Δ	Without Active Outreach	With Active Outreach	Δ
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.436	0.276	0.160	0.436	0.276	0.160
	(0.108)	(0.042)	(0.084)	(0.108)	(0.042)	(0.084)
Conditional First-Stage F	16.23	43.13		16.23	43.13	
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	0.757	2.004	-1.247	-0.234	-0.394	0.160
	(0.742)	(0.646)	(0.897)	(0.167)	(0.243)	(0.155)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	1.738	7.273	-5.535	-0.538	-1.430	0.892
	(1.841)	(2.572)	(2.702)	(0.437)	(0.894)	(0.647)
Conditional AR 95% CI	[-1.697, 7.407]	[2.401, 13.661]		[-1.964, 0.231]	[-3.487, 0.397]	
Incumbent Party	✓	✓		✓	✓	
Bolsa Família Intensity	✓	✓		✓	✓	
Municipality Characteristics	✓	✓		✓	✓	
Mayor Characteristics	✓	✓		✓	✓	
Benefit Value	✓	✓		✓	✓	
FPM Transfers	✓	✓		✓	✓	
Number of Mayors	1,394	1,394	1,394	1,394	1,394	1,394
Observations	2,788	2,788	2,788	2,788	2,788	2,788

Notes: Each outcome is estimated within 2008 *CRAS* municipalities in one fully interacted dual-IV system comparing units with and without active outreach; Δ is Without Active Outreach minus With Active Outreach. The instrument, treatment, and complete control set can vary across the two groups. Models use 2006-population weights and state-clustered standard errors. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). AR sets are plug-in conditional diagnostics, not exact joint weak-IV regions. Groups are predetermined scope conditions, not causal effects of adopting outreach.

day-to-day. The 2008 and 2009 *Censo SUAS* are annual snapshots recorded just before and just after the mayor elected in 2008 takes office. A change in the recorded manager between the two waves therefore identifies a replacement by the incoming administration. Exact CPF matches across waves also let us separate external hires from internal transfers.

The manager-turnover partition separates 318 *CRAS* municipalities with no observed turnover and 1,076 with an observed change. The first group includes municipalities without a matchable two-wave manager status, so it should not be read as verified continuity in every case. The first stage is clear in both groups and close in magnitude: 0.31 with observed turnover and 0.26 without an observed change. The corresponding vote-share 2SLS estimates are 5.92 and 3.06. The larger point estimate

Table 17: Heterogeneity by CRAS Intensity: *Proteção e Atendimento Integral à Família* (PAIF)

	Δ Incumbent's Vote Share			Δ Turnout		
	Without PAIF	With PAIF	Δ	Without PAIF	With PAIF	Δ
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.372	0.292	0.079	0.372	0.292	0.079
	(0.130)	(0.047)	(0.113)	(0.130)	(0.047)	(0.113)
Conditional First-Stage F	8.18	38.04		8.18	38.04	
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	0.653	2.251	-1.598	-0.442	-0.405	-0.037
	(0.570)	(0.791)	(1.004)	(0.279)	(0.204)	(0.162)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	1.758	7.697	-5.938	-1.189	-1.385	0.196
	(1.695)	(3.103)	(3.421)	(0.933)	(0.738)	(0.614)
Conditional AR 95% CI	[-1.501, 9.525]	[2.010, 15.783]		[-6.261, 0.324]	[-3.195, 0.050]	
Incumbent Party	✓	✓		✓	✓	
Bolsa Família Intensity	✓	✓		✓	✓	
Municipality Characteristics	✓	✓		✓	✓	
Mayor Characteristics	✓	✓		✓	✓	
Benefit Value	✓	✓		✓	✓	
FPM Transfers	✓	✓		✓	✓	
Number of Mayors	1,394	1,394	1,394	1,394	1,394	1,394
Observations	2,788	2,788	2,788	2,788	2,788	2,788

Notes: Each outcome is estimated within 2008 *CRAS* municipalities in one fully interacted dual-IV system comparing units with and without PAIF-tracked *Bolsa Família* families; Δ is Without PAIF minus With PAIF. The instrument, treatment, and complete control set can vary across the two groups. Models use 2006-population weights and state-clustered standard errors. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). AR sets are plug-in conditional diagnostics, not exact joint weak-IV regions. Groups are predetermined scope conditions, not causal effects of adopting PAIF services.

in observed-turnover municipalities is consistent with the alignment margin, but the difference between the two groups is imprecisely estimated.

Appendix E reports the full-sample three-cell version that restores municipalities without a *CRAS*, together with analogous versions for active outreach and PAIF.

The hire-type composition of those changes is informative. Across 3,357 manager changes in 2,583 municipalities, 75.50 percent involve a successor absent from the 2008 *CRAS* human-resources roster anywhere in the same municipality, 14.70 percent an internal transfer from another role at the same *CRAS*, and 9.80 percent a transfer from a different *CRAS* in the same municipality. The dominant case is fresh personnel rather than an internal reshuffle, which makes it unlikely that the turnover indicator captures only relabeling.

Table 18: Heterogeneity by CRAS Intensity: *Proteção e Atendimento Integral à Família* (PAIF) Volume

	Δ Incumbent's Vote Share			Δ Turnout		
	Below Median	Above Median	Δ	Below Median	Above Median	Δ
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.328	0.285	0.043	0.328	0.285	0.043
	(0.067)	(0.046)	(0.075)	(0.067)	(0.046)	(0.075)
Conditional First-Stage F	24.13	38.25		24.13	38.25	
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	-0.495	5.361	-5.856	-0.356	-0.384	0.028
	(0.958)	(1.407)	(2.026)	(0.350)	(0.137)	(0.343)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-1.509	18.778	-20.287	-1.085	-1.344	0.259
	(2.871)	(6.060)	(7.971)	(1.045)	(0.611)	(1.141)
Conditional AR 95% CI	[-7.706, 5.337]	[8.009, 35.047]		[-3.462, 1.278]	[-3.059, -0.306]	
Incumbent Party	✓	✓		✓	✓	
Bolsa Família Intensity	✓	✓		✓	✓	
Municipality Characteristics	✓	✓		✓	✓	
Mayor Characteristics	✓	✓		✓	✓	
Benefit Value	✓	✓		✓	✓	
FPM Transfers	✓	✓		✓	✓	
Number of Mayors	962	962	962	962	962	962
Observations	1,924	1,924	1,924	1,924	1,924	1,924

Notes: The sample contains 2008 *CRAS* municipalities reporting a positive number of *Bolsa Família* families followed through PAIF. Low and high volume are defined relative to the median positive family count in the headline sample; Δ is Below Median minus Above Median. The fully interacted dual-IV system allows the instrument, treatment, and complete control set to vary across the two groups. Models use the headline weights and state clustering. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016).

Table 19: Heterogeneity by CRAS Alignment: Manager Turnover

	Δ Incumbent's Vote Share			Δ Turnout		
	Without Turnover	With Turnover	Δ	Without Turnover	With Turnover	Δ
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.264	0.311	-0.047	0.264	0.311	-0.047
	(0.054)	(0.067)	(0.071)	(0.054)	(0.067)	(0.071)
Conditional First-Stage F	24.23	21.60		24.23	21.60	
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	0.809	1.844	-1.035	-0.655	-0.350	-0.305
	(0.858)	(0.657)	(1.279)	(0.343)	(0.190)	(0.236)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	3.061	5.922	-2.861	-2.478	-1.126	-1.353
	(3.201)	(2.612)	(4.570)	(1.172)	(0.697)	(0.801)
Conditional AR 95% CI	[-4.111, 10.408]	[1.442, 13.962]		[-5.079, 0.239]	[-3.182, 0.123]	
Incumbent Party	✓	✓		✓	✓	
Bolsa Família Intensity	✓	✓		✓	✓	
Municipality Characteristics	✓	✓		✓	✓	
Mayor Characteristics	✓	✓		✓	✓	
Benefit Value	✓	✓		✓	✓	
FPM Transfers	✓	✓		✓	✓	
Number of Mayors	1,394	1,394	1,394	1,394	1,394	1,394
Observations	2,788	2,788	2,788	2,788	2,788	2,788

Notes: Each outcome is estimated on all headline-sample municipalities with a 2008 *CRAS*. The groups compare municipalities with an observed 2008–2009 de facto manager change against the complement. The latter includes both confirmed nonchanges and municipalities without a matchable two-wave manager status, and is therefore labeled no observed turnover. The fully interacted dual-IV system allows the instrument, treatment, and complete control set to vary across the two groups; Δ is the first coefficient minus the second. Both group-specific coefficients are estimated in one joint model. Models use 2006-population weights and state-clustered standard errors. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). AR sets are plug-in conditional diagnostics, not exact joint weak-IV regions.

5.2.1 The Politics of Alignment

The decomposition suggests that the personnel margin matters most where the incoming administration replaced the inherited front-line manager. The next question is what this replacement represents. Are substitutes partisan loyalists selected into the post, bureaucrats who become politically aligned after appointment, or a different type of local actor whose subsequent behavior makes the office politically consequential?

Table 20: Manager Trajectory

	Outcomes per 1-SD Quota Shock			Number of Mayors	Mean
	(1)	(2)	(3)		
Panel A: Management and infrastructure outcomes					
Manager turnover	0.515	0.329	0.359	1,272	84.591
	(1.480)	(1.291)	(1.472)		
Opened a new *CRAS*	-1.803	-1.470	-1.390	1,394	8.680
	(1.759)	(1.434)	(1.509)		
Panel B: Prior affiliation among municipalities with turnover					
Affiliated with mayor's party	1.192	0.426	0.191	1,076	4.368
	(1.077)	(0.963)	(0.951)		
Affiliated with mayor's coalition party	1.662	0.609	0.012	1,076	7.249
	(1.536)	(1.020)	(1.026)		
Affiliated with any party	6.122	3.454	2.572	1,076	17.472
	(2.209)	(1.739)	(1.816)		
Panel C: Subsequent candidacy among municipalities with turnover					
Ran for office	6.581	4.054	3.479	1,076	10.874
	(2.873)	(1.355)	(1.342)		
Panel D: Subsequent alignment among at-risk substitutes					
Joined mayor's party (2009–2012)	-0.138	-0.234	-0.241	1,031	1.649
	(0.672)	(0.488)	(0.410)		
Joined mayor's coalition party (2009–2012)	-0.041	-0.164	-0.210	1,005	2.587
	(0.808)	(0.570)	(0.521)		
Incumbent Party	✓	✓	✓		
Bolsa Família Intensity	✓	✓	✓		
Municipality Characteristics		✓	✓		
Mayor Characteristics		✓	✓		
Benefit Value			✓		
FPM Transfers			✓		

Notes: Coefficients are percentage-point associations with a one-weighted-SD quota shock per 10,000 residents. Models use 2006-population weights and state-clustered standard errors. The table omits the no-control specification. Displayed columns are cumulative; Column (3) adds 2008 benefit value and FPM transfers. Panel A is timing-incompatible and Panels B–D condition on post-treatment manager status, so all rows are descriptive rather than causal.

Table 20 reports four municipality-level panels, using the *CRAS* and turnover definitions of Table 19. Each coefficient is the association between a one-standard-deviation

quota shock and the row outcome: management turnover and new *CRAS* openings in Panel A, and substitute-manager traits or actions in Panels B–D.

Does the shock induce management or infrastructure changes? Panel A provides little evidence of either response. The estimates for manager replacement and new *CRAS* openings are small relative to their uncertainty. The timing instead points to the 2008 mayoral transition as the source of turnover.

Was the substitute already politically tied to the mayor? Panel B provides little evidence that larger quota shocks brought in substitutes who were already affiliated with the mayor's party or coalition. The any-party row is positive, but it reflects broader political affiliation rather than a prior tie to the incoming mayor.

What does the substitute do next? Panel C shows the main behavioral response: municipalities exposed to a larger shock are more likely to have a substitute who later runs for office. The preferred coefficient is 3.48 percentage points, and 10.87 percent of turnover municipalities have a substitute who contests an election between 2010 and 2016.

Does the substitute realign with the mayor? Panel D restricts attention to substitutes not previously affiliated with the relevant mayoral party or coalition and asks whether they newly join it during the mayor's term. The estimates are small, and their confidence intervals include zero for both the mayor's party and the mayor's coalition.

Table 20 therefore points away from new infrastructure and prior partisan ties. The clearest response that grows with the size of the shock is the substitute's subsequent entry into electoral politics.

Table 21 then describes composition across manager types. External hires are no more likely to be party-affiliated than stayers or outgoing managers. Where they differ is subsequent electoral behavior. External hires run for office at 11.51 percent, compared with 5.92 percent for stayers and 6.99 percent for outgoing managers. The candidacies are overwhelmingly local: of the 119 substitutes who ran, 97 stood for *vereador*, with smaller numbers for vice mayor (18), mayor (10), and state deputy (2); the office counts exceed 119 because some ran for more than one office across 2010–2016.

Taken together, the evidence points to a form of bureaucratic politicization that develops through a voter-facing post rather than through the appointment of an established loyalist. This reading is close to Frey and Santarrosa (2026), who show that bureaucrats involved in *Bolsa Família* can accumulate political capital when the program expands. External incoming managers in our data are no more generally party-affiliated than stayers or outgoing managers, but they run for local office at roughly

Table 21: Substitute Manager Characteristics

Panel A. Comparison across managerial roles (incumbent units, 2008–2009)				
	# Managers	% Managers	Affiliated	Ran
Stayer	287	90.8%	18.1%	5.9%
Outgoing	1,216	90.1%	19.4%	7.0%
External Hire	938	87.3%	17.8%	11.5%
Internal Transfer	264	95.7%	10.2%	4.2%
Panel B. Offices sought by substitute managers who ran (TSE 2010–2016)				
	Level	# Managers	Runners	Substitutes
City council (*vereador*)	Municipal	97	81.5%	8.07%
Vice mayor	Municipal	18	15.1%	1.50%
Mayor (*prefeito*)	Municipal	10	8.4%	0.83%
State deputy	State	2	1.7%	0.17%
Any office		119	100.0%	9.90%

Notes: The table is restricted to the exact locked headline municipality sample. Panel A aggregates manager-unit records at persistent incumbent *CRAS* units observed in both the 2008 and 2009 *Censo SUAS* waves. *Stayer* is the same manager in both years; *Outgoing* is the 2008 manager who was replaced; *External Hire* is a replacement absent from the municipality's 2008 *CRAS* human-resources roster; and *Internal Transfer* is a replacement present in that roster. # Managers counts managers distinctly within each municipality and then sums those counts across the headline sample; a manager serving multiple municipalities is therefore counted once in each. % Managers divides that count by manager-unit observations. *Affiliated* is the share with an active party affiliation on 1 January 2009. *Ran* is the share appearing as a candidate in the TSE 2010–2016 candidate data. Panel B reports office categories among the 119 substitute managers who ran. *Runners* uses those candidates as the denominator; *Substitutes* uses all 1,202 replacement managers, using the same within-municipality counting rule. A manager may appear in more than one office category, so the office shares need not sum to 100 percent.

twice the rate of stayers, and this response rises with the quota shock. In the model's language, replacing the inherited manager may raise the multiplier on discretionary effort a_m once a functioning *CRAS* apparatus exists. The stepping-stone label refers to the manager's later political trajectory, not to a claim about prior loyalty or the mayor's reason for hiring that person.

6 Conclusion

This paper asks whether locally elected officials can obtain electoral returns from a welfare program whose formal design is centralized. *Bolsa Família* provides a useful setting because the federal government determines eligibility, benefit values, and payments, while municipalities register eligible households. Federal quota revisions during the 2009–2012 mayoral term generated municipality-level variation in authorized expansion while leaving the national program rules unchanged.

Federal authorization produced both realized enrollment and electoral gains. A one-standard-deviation quota shock corresponds to 171.50 additional authorized slots per 10,000 residents and induces 54.68 additional beneficiary families per 10,000 residents. A one-standard-deviation increase in instrumented enrollment corresponds to

181.36 additional beneficiary families per 10,000 residents and raises vote share by 4.69 percentage points. In natural units, 100 additional beneficiary families per 10,000 residents raise vote share by 2.59 percentage points. These are returns to the expansion moved by the federal quota changes.

The first capacity result concerns political returns rather than enrollment pass-through. The first-stage coefficients are 0.30 in municipalities with a pre-existing *CRAS* and 0.32 in municipalities without one. The 2SLS estimate in the *CRAS* group is 6.11; outside that group, the confidence interval includes zero. Pre-existing infrastructure did not determine whether authorization became enrollment. It conditioned where realized expansion generated a clear electoral return.

How the local apparatus operated also matters in the IV point estimates. Within *CRAS* municipalities, the 2SLS vote-share estimate is 7.27 with active outreach, while the estimate without it is close to zero. The corresponding estimates are 7.70 where offices served *Bolsa Família* families through PAIF and close to zero where they did not. Among offices reporting positive PAIF family counts, the estimate is 18.78 above the median service volume, while the lower-volume estimate is imprecise. We read these as scope patterns: electoral returns are larger where the local welfare apparatus reaches and works more intensively with beneficiary families.

Manager turnover adds a personnel margin to this pattern. Within *CRAS* municipalities, the first stage is similar with and without an observed manager change. The vote-share 2SLS estimate is 5.92 with observed turnover and 3.06 without it. The point estimate is larger with turnover, but the difference between the two groups is imprecisely estimated.

The trajectory evidence narrows the interpretation of that personnel margin. We find little evidence that the quota shock changed manager turnover, new *CRAS* openings, prior ties between substitutes and the mayor's party, or later partisan realignment. Their electoral behavior differs. Among turnover municipalities, 10.87 percent have a substitute who later runs for office, and a one-standard-deviation quota shock is associated with a 3.48 percentage-point increase in that probability. The pattern is consistent with the managerial post providing a route into local politics rather than with the appointment of prior partisan loyalists.

The usual distinction between centralized policy and local politics is incomplete in this setting. Federal authorization expanded enrollment in municipalities with and without a *CRAS*, but a positive electoral response appeared only where a pre-existing welfare office was present. Within those offices, active outreach, service intensity, and

manager turnover describe where the response is largest or most clearly identified. Centralized rules can constrain discretion over eligibility and benefit values. They cannot remove local politics from program access while enrollment still requires municipal action.

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Appendix

B Data Sources and Construction

Figure B.1: Family Registration Cohorts in the 2012 *Cadastro Único* Snapshot, by Reported Income



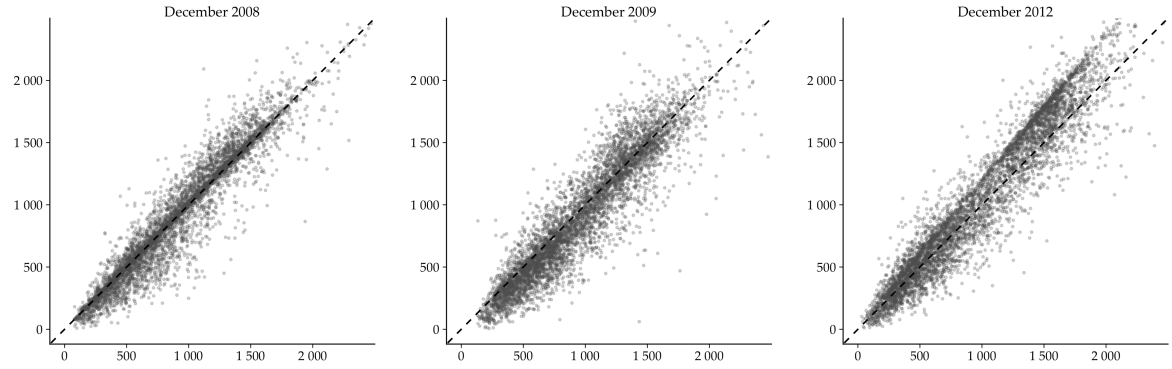
Notes: Counts sum municipality-level family records in the 2012 snapshot. "2008 or earlier" pools all valid registration dates through December 2008. Municipality and nominal monthly per-capita family income are recorded in 2012; income is not measured at registration. Registration dates outside 1900–2012 are excluded. The at-most-R$140 line is the sum of the at-most-R$70 and above-R$70-to-at-most-R$140 lines, so these displayed categories overlap by construction. Families with missing or invalid income are retained in the restricted municipal sidecar but omitted from the four income lines; out-of-window registration dates are reported in the diagnostic sidecar.

Table B.1: Baseline Characteristics — Full Distributional Detail

	N	Min	P10	P25	Mean	Median	P75	P90	Max	SD
Population (log)	2,105	6.678	8.084	8.583	9.359	9.264	9.942	10.725	15.569	1.074
Gini (%)	2,105	30.000	46.000	51.000	54.839	55.000	59.000	63.000	80.000	6.727
Share poor (%)	2,105	7.660	33.656	48.090	64.100	66.780	82.980	88.526	99.000	20.976
Income per capita (R$)	2,105	69.200	128.4	172.0	337.2	300.1	461.4	600.4	1,759.8	195.1
Share formal workers (%)	2,105	2.890	13.016	19.790	35.785	33.990	50.150	62.116	86.380	18.280
Urban share (%)	2,105	0.000	27.148	40.958	58.769	59.197	77.680	90.946	100.0	23.298
HDI (IDHM)	2,105	0.246	0.383	0.434	0.522	0.528	0.607	0.658	0.791	0.105
Expected years of schooling	2,105	2.820	5.864	6.970	8.284	8.360	9.740	10.530	13.020	1.800
Life expectancy at birth (years)	2,105	57.810	62.624	65.130	68.290	68.800	71.350	73.266	77.240	4.011
Illiteracy rate (15+ years, %)	2,105	0.910	7.512	11.790	21.979	18.740	32.000	40.018	59.950	12.495

Notes: Distributional statistics for baseline characteristics of the 2,105 municipalities in the estimation sample (one observation per municipality). All variables are drawn from the *Atlas do Desenvolvimento Humano no Brasil* (PNUD) using 2000 Demographic Census microdata as the underlying source. Gini and urban share are displayed multiplied by 100 to align with the other percentage-scale variables. Population is reported as a natural logarithm. These Census-2000 characteristics describe the compatibility sample; they are not the current headline control vector. The main design uses its locked sample and predetermined municipality and mayor controls. All rows here are retained as distributional diagnostics.

Figure B.2: Authorized Quota vs. Realized Enrollment, 2008, 2009, and 2012



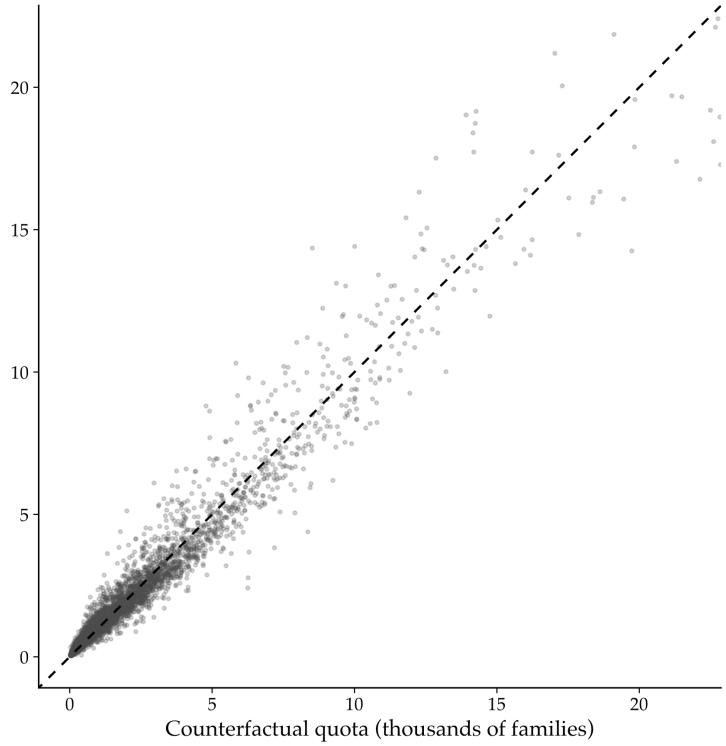
Note. Each dot is a municipality. The horizontal axis shows the federally authorized quota and the vertical axis shows the twelve-month moving average of realized *Bolsa Família* enrollment (in thousands). The dashed line marks the 45-degree locus of full take-up. December 2008 uses the pre-reform quota (*EST_2006*); December 2009 and December 2012 use the respective post-reform quotas. The rightward shift of the scatter in December 2009 relative to December 2008 reflects the quota expansion; by December 2012 most municipalities have closed the gap, as enrollment caught up with the expanded ceilings. Both axes are capped at the 99th percentile of the cross-municipal distribution across all three years; large urban municipalities above this threshold are excluded from the frame but not from any analysis.

Table B.2: Variant Diagnostic: Role of Each Baseline Characteristic

	Gini	Share Poor (%)	Income (R$)	Formal Workers (%)	Population (log)	Urban Share
Panel A: Adding one baseline × Post at a time						
Vote Share: $\widehat{\beta}_1$	4.433	4.082	4.194	4.405	6.095	4.677
	(0.983)	(1.530)	(1.968)	(1.403)	(1.570)	(1.281)
Winning: $\widehat{\beta}_1$	9.438	7.034	5.765	8.009	8.022	8.511
	(1.135)	(1.386)	(1.601)	(1.380)	(2.252)	(1.375)
Kleibergen–Paap F	39.85	26.48	32.01	27.82	40.65	28.18
Panel B: All baselines × Post except the one named						
Vote Share: $\widehat{\beta}_1$	5.084	4.901	5.202	4.931	3.886	4.877
	(1.471)	(1.482)	(1.540)	(1.482)	(1.813)	(1.471)
Winning: $\widehat{\beta}_1$	6.000	5.269	6.455	5.294	4.969	5.107
	(1.940)	(2.066)	(2.238)	(1.840)	(1.727)	(1.917)
Kleibergen–Paap F	73.74	67.23	64.13	72.39	51.31	74.41

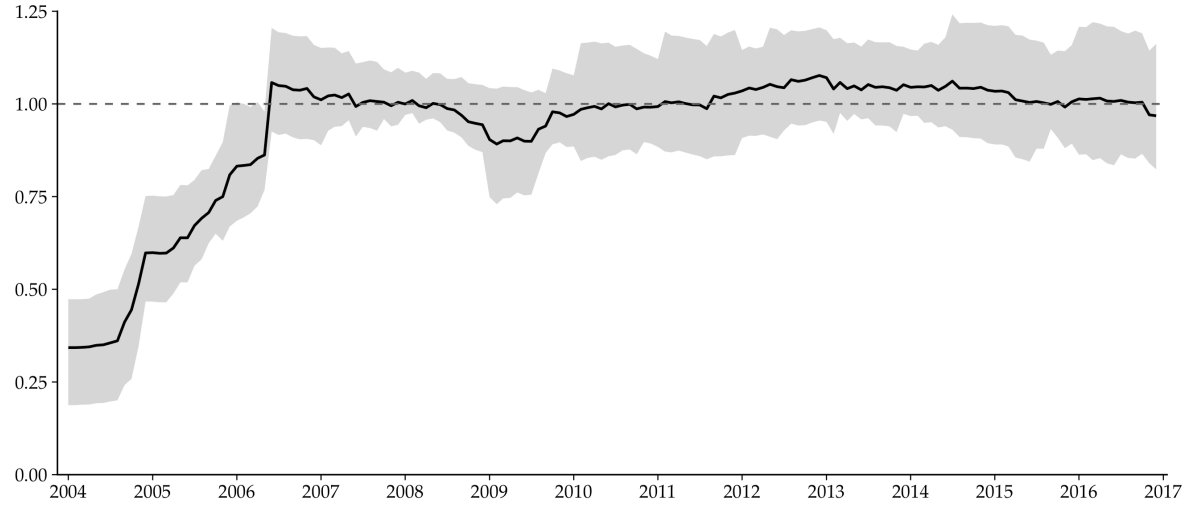
Notes: Each cell reports an instrumental-variables estimate of the effect of one standard deviation of realized *Bolsa Família* expansion on the stated outcome, with cluster-robust standard errors at the state level in parentheses below. Panel A adds each baseline characteristic to the canonical fixed-effects specification (year, municipality, and pre-reform-winner party) one at a time (each column is a separate regression that includes only the single baseline × Post named in the column header). Panel B runs the opposite test: each column includes all six baselines × Post *except* the one named in the column header. Column headers identify the six baseline characteristics in their original units: Gini is an index in [0,1]; shares of poor, formal workers, and urban population are reported as percentages; income per capita is in 2000 R$; population enters as a natural logarithm. All baselines are interacted with the post-reform indicator Post_t. All columns include the canonical fixed effects (year, municipality, and pre-reform-winner party) so the only object varying across columns is the baseline-characteristic bundle. The Kleibergen–Paap first-stage F-statistic is reported once per column; it is identical for the Vote Share and Winning regressions since the first stage does not depend on the outcome. Sample: 2,105 municipalities × 2 election years.

Figure B.3: Actual 2009 Quota vs. Counterfactual Quota



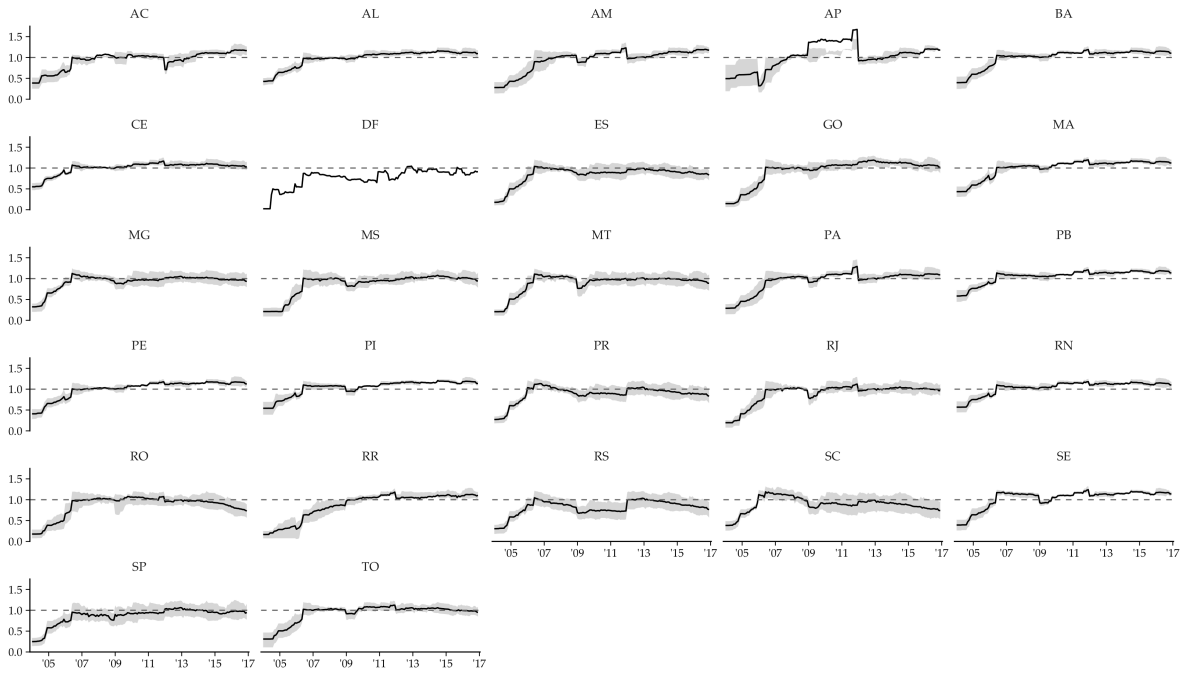
Note. Each dot is a municipality. The horizontal axis plots the counterfactual 2009 quota (the quota that would have prevailed under the pre-reform allocation formula applied to 2006 PNAD data, with poverty cutoffs of R$90 per capita on the 2000 Census and R$137 per capita on the 2006 PNAD). The vertical axis plots the actual 2009 quota. Both are in thousands of authorized families. This short-window comparison is distinct from the main instrument, which uses actual 2012 authorization. The dashed line is the 45-degree locus of zero shock: points above received more than the pre-reform formula would have given, points below received less. Axes are capped at the 99th percentile of the joint distribution.

Figure B.4: Realized vs. Authorized Enrollment, Unweighted



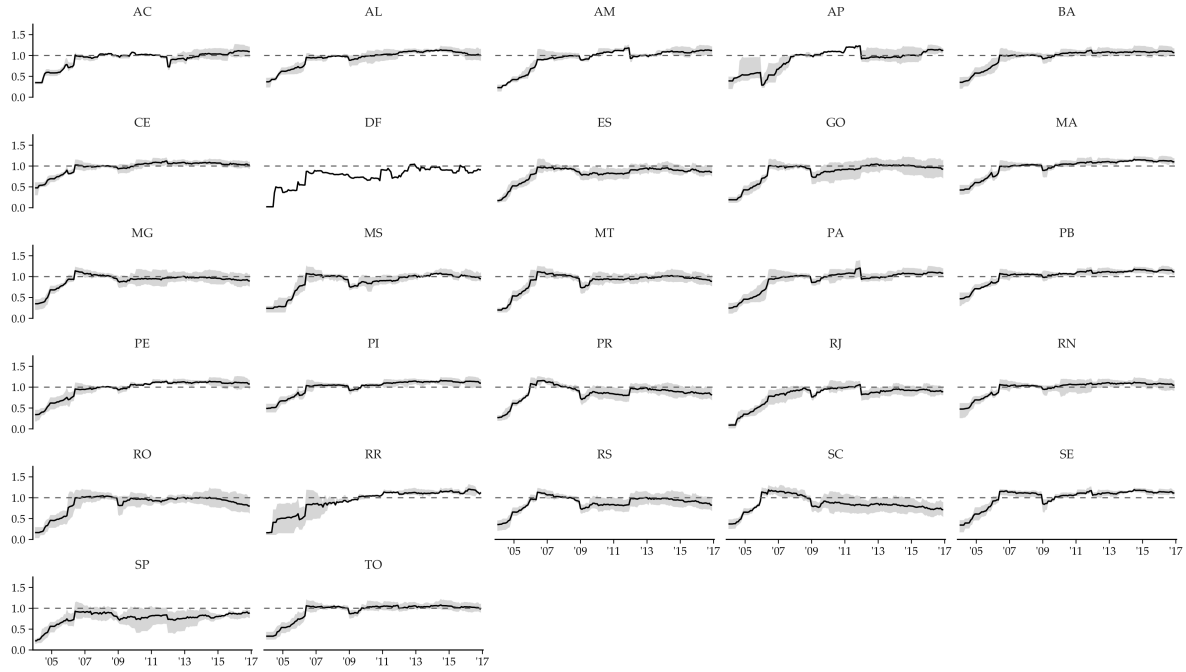
Note: Companion to Figure 1. Each point on the line is the simple cross-municipal mean of the ratio of realized *Bolsa Família* beneficiaries to the authorized quota in a given month, weighting every municipality equally regardless of size. The shaded band spans the 25th to 75th percentile of the cross-municipal distribution. The dashed horizontal line marks full take-up (ratio = 1). The unweighted view is dominated by small municipalities, many of which report realized enrollment above their authorized ceiling, which pushes the simple cross-municipal mean above one for most of the sample period.

Figure B.5: Realized vs. Authorized Enrollment by State, Unweighted



Note. Each panel is one state. Within each state, the black line is the simple cross-municipal mean of realized/authorized at each date, and the shaded band spans the 25th to 75th percentile across municipalities within the state. The dashed horizontal line marks full take-up (ratio = 1). The vertical scale is common across all state panels to facilitate comparison.

Figure B.6: Realized vs. Authorized Enrollment by State, Weighted by Quota Size



Note. Companion to Figure B.5. Each panel is one state. The black line is the within-state quota-weighted mean of realized/authorized at each date, and the shaded band spans the weighted 25th and 75th percentiles. The dashed horizontal line marks full take-up (ratio = 1). Weighting by quota size shifts the within-state average toward large municipalities, which typically sit at or below the authorized ceiling.

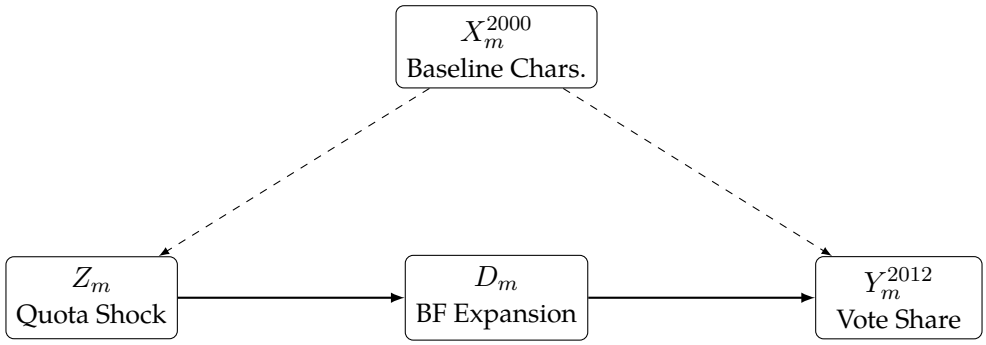


Figure B.7: Identification DAG. Solid arrows are the causal path of interest ($Z \to D \to Y$). Dashed arrows are back-door paths closed by including baseline characteristics $\times$ Post as controls. The 2008 incumbent vote share is omitted from the controls: with municipality fixed effects, the first-differenced specification already differences it out.

C Robustness Exhibits

Table C.1: Turnout placebo: the federal shock does not move the extensive margin

Outcome	Mean	SD	RF $\hat{\beta}$	RF p	IV $\hat{\beta}$	IV SE	AR 95% set
Turnout (voted/registered, pp)	87.25	5.52	-0.005	0.866	-0.025	0.147	[-0.343, 0.283]
log(voted)	9.04	1.01	-0.000	0.553	-0.001	0.002	[-0.005, 0.003]
log(registered voters)	9.18	1.04	-0.000	0.732	-0.001	0.002	[-0.006, 0.004]
Headline: incumbent vote share (pp)	51.41	12.05	0.986	<0.001	4.989	1.433	[2.341, 8.478]
Incumbent votes/voted (pp)	47.39	9.99	0.913	<0.001	4.618	1.321	[2.162, 7.813]
Incumbent votes/registered voters (pp)	41.44	9.44	0.747	<0.001	3.781	1.152	[1.627, 6.553]

Note. Every row uses the locked central-specification sample: R$90/137 counterfactual-quota shock; raw levels; standardized on the locked comparison sample; municipality, year, and incumbent-party fixed effects; 5 centrally declared baseline controls interacted with post; weighted by Census 2000 population; standard errors clustered by state. The Anderson–Rubin sets invert the cluster-robust test. The source field `could` is registered voters; the recovered legacy producer mislabeled its logarithm as registered minus voted. Sample: $N = 4,210$ municipality-year observations from $2,105$ municipalities.

Table C.2: Robustness Checks: Turnout

	Alt. Outcomes		Alt. Treatment	Alt. Controls	Alt. Weights	
	Main	Winsorized	Valid Votes Cast	Short Window	Vote Share in 2008	IPW
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.301	0.301	0.301	0.292	0.301	0.302
	(0.052)	(0.052)	(0.052)	(0.049)	(0.052)	(0.049)
Kleibergen–Paap F	33.30	33.30	33.30	35.53	33.00	37.68
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	-0.354	-0.358	-0.060	-0.181	-0.351	-0.353
	(0.180)	(0.176)	(0.227)	(0.103)	(0.181)	(0.185)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-1.173	-1.189	-0.198	-0.620	-1.167	-1.172
	(0.639)	(0.629)	(0.753)	(0.353)	(0.644)	(0.643)
AR 95% CI	[-2.792, 0.053]	[-2.787, 0.014]	[-1.885, 1.434]	[-1.441, 0.112]	[-2.803, 0.068]	[-2.738, 0.090]
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics	✓	✓	✓	✓	✓	✓
Mayor Characteristics	✓	✓	✓	✓	✓	✓
Benefit Value	✓	✓	✓	✓	✓	✓
FPM Transfers	✓	✓	✓	✓	✓	✓
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: The table varies the turnout outcome, treatment window, lag control, and weighting scheme. All columns use the complete control set and state-clustered standard errors.

Table C.3: Electoral-Margin Estimates

	Δ Incumbent's Vote Share		
	(1)	(2)	(3)
Panel A: Reduced Form			
ΔQuota_m (ρ_1)	1.929	1.299	1.414
	(0.604)	(0.461)	(0.482)
Panel B: Two-Stage Least Squares			
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	6.082	3.619	4.690
	(2.322)	(1.569)	(1.991)
AR 95% CI	[2.019, 12.834]	[0.876, 7.978]	[1.242, 10.282]
Incumbent Party	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓
Municipality Characteristics		✓	✓
Mayor Characteristics		✓	✓
Benefit Value			✓
FPM Transfers			✓
Number of Mayors	2,078	2,078	2,078
Observations	4,156	4,156	4,156

Notes: The dependent variable is the 2012–2008 change in the 2008 winner's share of valid candidate votes. Panel A reports the reduced form and Panel B reports 2SLS. Specification, weights, fixed effects, controls, and state clustering match the headline tables.

Table C.4: Alternative Instrument Thresholds

	Central specification	R$90/120	R$90	Raw Δ
Panel A: Vote Share				
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	4.989	2.935	2.129	1.981
	(1.433)	(1.105)	(1.024)	(1.230)
Anderson-Rubin 95% CI	[2.35, 8.45]	[0.75, 5.25]	[-0.05, 4.15]	[-1.15, 4.25]
Panel B: Winning				
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	5.673	4.074	3.407	0.925
	(1.935)	(1.352)	(1.514)	(4.299)
Anderson-Rubin 95% CI	[1.75, 9.95]	[1.40, 6.95]	[0.30, 6.50]	[-11.95, 7.75]
Kleibergen-Paap F	74.16	1256.91	1692.37	37.43
Electoral Covariates	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210

Notes: Each column uses a different counterfactual-quota instrument. R$90 uses the single 2009 poverty line; R$90/120 and R$90/137 use dual cutoffs reflecting the 2009 *Informe* n.169 values; "Raw Δ" is the mechanical difference between the 2009 and 2008 quotas without counterfactual adjustment. The preferred specification in the main text uses R$90/137. All specifications use the preferred Col 3 control set, pop. 2000 weights, and state-clustered SE.

Table C.5: Alternative Endogenous-Variable Windows

	3-month	6-month	9-month	Central specification	18-month
Panel A: Vote Share					
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	5.476	5.847	5.048	4.989	5.058
	(2.178)	(2.144)	(1.622)	(1.433)	(1.264)
Anderson-Rubin 95% CI	[2.15, 12.70]	[2.40, 12.15]	[2.25, 9.25]	[2.35, 8.45]	[2.60, 7.95]
Panel B: Winning					
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	6.227	6.649	5.740	5.673	5.752
	(2.112)	(2.297)	(1.957)	(1.935)	(2.024)
Anderson-Rubin 95% CI	[2.05, 11.65]	[2.10, 12.15]	[1.80, 10.10]	[1.75, 9.95]	[1.75, 10.35]
Kleibergen-Paap F	25.22	37.54	61.10	74.16	65.59
Electoral Covariates	✓	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210	4,210

Notes: Each column uses a different moving-average window to define the endogenous realized-expansion variable, centered on the 2009 policy window. Preferred column in the main text is 12-month. All specifications use the preferred Col 3 control set.

Table C.6: Alternative Outcome Definitions

	Votes/Valid	Votes/Cast	Votes/Eligible	Margin
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	4.989	4.618	3.781	2.785
	(1.433)	(1.321)	(1.152)	(2.083)
Anderson-Rubin 95% CI	[2.35, 8.45]	[2.20, 7.80]	[1.65, 6.55]	[-1.20, 7.60]
Kleibergen-Paap F	74.16	74.16	74.16	74.16
Electoral Covariates	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210

Notes: Each column uses a different denominator for incumbent vote share. "Votes/Valid" (preferred) uses valid candidate votes; "Votes/Cast" uses total cast ballots including blank and null; "Votes/Eligible" uses the total eligible electorate; "Margin" reports the vote margin in percentage points. All specifications use the preferred Col 3 control set.

Table C.7: Alternative Observation Weights

	Pop 2000	Pop 2002	Pop 2004	Pop 2008	Unweighted
Panel A: Vote Share					
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	4.989	5.044	5.097	5.156	6.454
	(1.433)	(1.445)	(1.458)	(1.486)	(5.791)
Anderson-Rubin 95% CI	[2.35, 8.45]	[2.40, 8.55]	[2.45, 8.65]	[2.45, 8.80]	[-15.00, 25.00]
Panel B: Winning					
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	5.673	5.888	6.090	6.096	-2.053
	(1.935)	(1.925)	(1.922)	(2.017)	(20.247)
Anderson-Rubin 95% CI	[1.75, 9.95]	[2.05, 10.20]	[2.30, 10.40]	[2.10, 10.65]	[-15.00, 25.00]
Kleibergen-Paap F	74.16	71.78	69.61	68.56	1.76
Electoral Covariates	✓	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210	4,210

Notes: Each column uses a different weighting scheme for the preferred 2SLS. Pop 2000 is the main specification; Pop 2002 is interpolated linearly between the 2000 and 2004 Census vintages; Pop 2004 and Pop 2008 are IBGE inter-census estimates; "Unweighted" omits weights entirely. All specifications otherwise use the preferred Col 3 control set.

Table C.8: Robustness to State-Specific Election-Cycle Shifts

	Δ Incumbent's Vote Share			
	(1)	(2)	(3)	(4)
Panel A: First Stage				
ΔQuota_m (π_1)	0.317	0.359	0.301	0.487
	(0.063)	(0.062)	(0.052)	(0.041)
Kleibergen–Paap F	25.43	33.63	33.30	138.27
Panel B: Reduced Form				
ΔQuota_m (ρ_1)	1.929	1.299	1.414	0.731
	(0.604)	(0.461)	(0.482)	(0.338)
Panel C: Two-Stage Least Squares				
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	6.082	3.619	4.690	1.499
	(2.322)	(1.569)	(1.991)	(0.697)
AR 95% CI	[2.019, 12.834]	[0.876, 7.978]	[1.242, 10.282]	[0.072, 2.990]
Incumbent Party	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓
Municipality Characteristics		✓	✓	✓
Mayor Characteristics		✓	✓	✓
Benefit Value			✓	✓
FPM Transfers			✓	✓
State × Post				✓
Number of Mayors	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156

Notes: Columns (1)–(3) reproduce the headline specifications. Column (4) adds state-by-post interactions to Column (3), absorbing a separate 2008–2012 electoral shift for each state. All columns use 2006-population weights and state-clustered standard errors. AR sets use cluster-robust quadratic inversion.

Table C.9: Winsorization of the Outcome Variable

	None	1/99	2/98	5/95
Panel A: Vote Share				
$\widehat{\Delta\text{Beneficiaries}_m}$ (β_1)	4.989	4.429	4.223	4.130
	(1.433)	(1.251)	(1.181)	(1.004)
Anderson-Rubin 95% CI	[2.35, 8.45]	[2.10, 7.40]	[2.00, 7.00]	[2.25, 6.50]
Panel B: Winning				
$\widehat{\Delta\text{Beneficiaries}_m}$ (β_1)	5.673	5.673	5.673	5.673
	(1.935)	(1.935)	(1.935)	(1.935)
Anderson-Rubin 95% CI	[1.75, 9.95]	[1.75, 9.95]	[1.75, 9.95]	[1.75, 9.95]
Kleibergen-Paap F	74.16	74.16	74.16	74.16
Electoral Covariates	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210

Notes: Each column winsorizes the outcome variable at the indicated lower/upper percentile pair. "None" reproduces the preferred specification. Winsorization bounds the influence of outlier observations in the vote-share distribution.

Table C.10: Alternative Specifications Using the 2012 Quota Revision

	R$90	R$90/120	R$90/137	R$140
Panel A: Vote Share				
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	1.306	1.497	1.641	18.270
	(0.494)	(0.497)	(0.499)	(13.793)
Anderson-Rubin 95% CI	[0.30, 2.30]	[0.50, 2.50]	[0.70, 2.70]	[-2.65, 25.00]
Panel B: Winning				
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	1.985	2.160	2.294	78.490
	(0.779)	(0.891)	(1.004)	(50.956)
Anderson-Rubin 95% CI	[0.40, 3.55]	[0.40, 4.05]	[0.35, 4.45]	[21.25, 25.00]
Kleibergen-Paap F	4339.69	1618.93	625.92	2.05
Electoral Covariates	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210

Notes: Each column uses an instrument constructed from the 2012 quota revision (rather than the 2009 revision used in the main text), under a different income threshold for the counterfactual. The endogenous variable is the realized-expansion moving average through December 2012. R$140 is the 2012 *Bolsa Família* eligibility threshold; the other three are inherited from the 2009 eligibility regime for comparability. The main-text results rely on the 2009 reform; this table confirms the qualitative findings survive the alternative identifying window.

Table C.11: Instrument Diagnostics and OLS/IV Comparison

	Vote Share	Winning
OLS (β^{OLS})	2.255	1.828
	(0.985)	(1.814)
2SLS (β^{IV})	4.989	5.673
	(1.433)	(1.935)
Kleibergen-Paap F	74.16	74.16
Wu-Hausman $\chi^2(1)$	12.94	4.49
Wu-Hausman p-value	0.001	0.044
Observations	4,210	4,210

Notes: Instrument diagnostics for the preferred Col 3 specification. The Kleibergen-Paap F-statistic is the squared t-ratio of the shock in the first-stage with cluster-robust SE. Wu-Hausman is the $\chi^2(1)$ test of exogeneity via a control-function regression. The OLS column reports the uninstrumented coefficient of the endogenous variable. All specifications include year and municipality fixed effects, electoral covariates, and baseline characteristics $\times$ post. Weights: pop. 2000. Cluster-robust SE at the state level.

Table C.12: Alternative Standard Error Clustering

	State	Municipality
Panel A: Vote Share		
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	4.989	4.989
	(1.433)	(1.576)
Panel B: Winning		
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	5.673	5.673
	(1.935)	(3.930)
Number of clusters	26	2,105
Observations	4,210	4,210

Notes: 2SLS point estimates are identical across columns; only standard errors vary. Columns are ordered from largest geographic unit (state) to smallest (municipality). All specifications use the preferred Col 3 control set and pop. 2000 weights. Mesoregion and microregion clustering are omitted when the corresponding IBGE codes are not present in the dataset; Conley spatial standard errors likewise require muni-level coordinates, which are outside the current pipeline.

Table C.13: Leave-One-State-Out Jackknife

	Vote Share		Winning		
State dropped	β_1	SE	β_1	SE	Obs.
AC	4.997	(1.433)	5.721	(1.940)	4,194
AL	5.016	(1.454)	5.611	(1.927)	4,128
AM	4.995	(1.432)	5.669	(1.945)	4,158
AP	5.000	(1.417)	5.574	(1.896)	4,200
BA	3.954	(1.498)	3.833	(1.647)	3,822
CE	4.952	(1.472)	5.654	(1.951)	4,094
ES	4.622	(1.497)	5.389	(1.905)	4,148
GO	5.126	(1.353)	5.398	(1.852)	4,006
MA	5.181	(1.463)	6.515	(1.889)	4,018
MG	4.355	(2.315)	9.671	(3.571)	3,610
MS	4.990	(1.430)	5.631	(1.931)	4,164
MT	5.013	(1.413)	5.616	(1.960)	4,114
PA	4.768	(1.409)	5.274	(1.935)	4,090
PB	4.972	(1.441)	5.443	(1.899)	4,052
PE	5.012	(1.446)	6.744	(1.624)	4,042
PI	5.038	(1.455)	5.797	(1.940)	4,076
PR	4.947	(1.447)	5.565	(1.960)	3,952
RJ	15.632	(9.999)	19.534	(11.403)	4,132
RN	4.964	(1.439)	5.598	(1.941)	4,086
RO	5.001	(1.441)	5.698	(1.946)	4,164
RR	4.992	(1.433)	5.692	(1.940)	4,192
RS	4.975	(1.420)	5.521	(1.963)	3,860
SC	5.037	(1.464)	6.004	(1.986)	3,958
SE	5.015	(1.452)	5.678	(1.956)	4,146
SP	6.542	(0.902)	5.899	(2.670)	3,736
TO	5.067	(1.432)	5.836	(1.956)	4,104
Main estimate (full sample)	**4.989**	**(1.433)**	**5.673**	**(1.935)**	**4,210**

Notes: Each row reports the preferred 2SLS estimate after excluding the indicated state from the estimation sample. The last row reproduces the full-sample main estimate from Table 9 for comparison. All specifications use the preferred Col 3 control set, population-2000 weights, and state-clustered standard errors.

Table C.14: Dropping the top-K municipalities by absolute identifying weight

Sample	N_{obs}	N_{munis}	$\hat{\pi}_{\text{FS}}$	First-stage F	$\hat{\beta}_{\text{IV}}$	SE	AR 95% set
Full sample	4,210	2,105	0.198	74.16	4.99	1.43	[2.341, 8.478]
Drop top 5 by $\lvert w_m \rvert$	4,200	2,100	0.025	1.67	14.50	21.54	$[-\infty, +\infty]$
Drop top 10 by $\lvert w_m \rvert$	4,190	2,095	0.031	2.27	21.44	20.91	$[-\infty$, -48.823] $\cup$ [-9.061, $+\infty]$
Drop top 20 by $\lvert w_m \rvert$	4,170	2,085	-0.010	0.44	-84.51	156.02	$[-\infty, +\infty]$
Drop top 50 by $\lvert w_m \rvert$	4,110	2,055	-0.018	1.05	-58.16	85.23	$[-\infty$, 25.428] $\cup$ [63.604, $+\infty]$
Drop top 100 by $\lvert w_m \rvert$	4,010	2,005	-0.009	0.28	-52.81	122.62	$[-\infty, +\infty]$

Note. Municipalities are ranked by the absolute weighted Frisch–Waugh–Lovell instrument–treatment covariance contribution under the central specification: R$90/137 counterfactual-quota shock; raw levels; standardized on the locked comparison sample; municipality, year, and incumbent-party fixed effects; 5 centrally declared baseline controls interacted with post; weighted by Census 2000 population; standard errors clustered by state. Each remaining sample is re-estimated under the same specification. The treatment and instrument retain their full-sample standardization so coefficients remain in common units across exclusions. Anderson–Rubin 95% sets are computed for every row by exact quadratic inversion of the cluster-robust test. The smallest number of municipalities accounting for half of absolute identifying weight is 7.

Table C.15: Dose-Level Response: Binned Means and Linearity Test

Shock decile	n	Mean shock (std)	Δ realized (std)	Δ vote share (pp)
1	211	-3.555	0.347	-6.866
2	211	-0.384	0.041	-2.699
3	211	-0.163	0.030	-1.739
4	211	-0.059	0.015	-2.220
5	211	-0.002	0.027	0.794
6	210	0.045	0.011	-6.745
7	210	0.092	0.013	-2.493
8	210	0.168	0.020	-4.123
9	210	0.322	0.048	-0.224
10	210	5.982	1.986	1.950

Linear fit: Δ vote share = 0.548 $\times$ shock (std), SE 0.094
R^2 linear = 0.1601; R^2 decile fixed effects = 0.1524

Notes: Municipalities are sorted into deciles by standardized quota shock. For each decile, the table reports the number of municipalities, the population-weighted mean of the standardized shock, and the population-weighted mean of the 2008-to-2012 change in realized expansion and in incumbent vote share. A linear regression of Δ vote share on shock (weighted by 2000 population, with state fixed effects) is reported together with a decile-fixed-effect specification of the same dependent variable. The comparison of R^2 between the two specifications tests whether the dose-response departs from linearity.

Table C.16: Counterfactual Flip Simulation

Panel A. Aggregate effect on 2012 reelection rate	
Observed winning rate	56.3%
Counterfactual winning rate (no 2009 reform)	48.9%
Share of municipalities with flipped outcome	8.79%
Share weighted by 2000 population	21.70%

Panel B. Flips concentrated in close races			
Threshold |margin|	n	% of sample	% flipped
<1 pp	170	8.1%	20.0%
<2 pp	339	16.1%	17.1%
<3 pp	511	24.3%	14.1%
<5 pp	818	38.9%	12.6%

Notes: Panel A compares the observed 2012 reelection rate with a counterfactual rate in which the reform-induced vote-share effect is subtracted. The counterfactual is $\widehat{\text{VS}}_{m,\text{no reform}} = \text{VS}_{m,2012} - \hat{\rho}_1 \cdot Z_m$, using the intention-to-treat coefficient $\hat{\rho}_1 \approx 1.00$ reported in Section 4 and Z_m the standardized quota shock received by municipality m. A municipality is coded as flipped when the incumbent's winning indicator differs between the observed and counterfactual scenarios at the 50% threshold. Panel B restricts the calculation to municipalities decided by increasingly narrow margins, showing that flipped outcomes concentrate in close races.

Table C.17: Instrument Comparison: 90_137 (preferred) vs 90_120 (alternative)

	90_137 (preferred)				**90_120** (alternative)			
Sample	$\hat{\beta}$	SE	KP F	AR 95% CI	$\hat{\beta}$	SE	KP F	AR 95% CI
Full sample	4.989	1.433	74.16	[2.35, 8.45]	2.935	1.105	1256.91	[0.75, 5.25]
Drop RJ state	15.632	9.999	4.86	[4.00, 25.00]	3.701	2.141	32.00	[0.05, 9.85]
Drop Rio-capital	16.525	11.119	4.56	[3.90, 25.00]	4.117	2.537	28.57	[-0.05, 11.75]
Drop RJ interior	4.808	1.366	73.71	[2.35, 8.15]	2.809	1.048	1418.75	[0.75, 5.00]

Notes: Outcome is incumbent vote share. Each row reports the preferred Section 4 2SLS specification (municipality, year, and incumbent-party fixed effects; baseline characteristics $\times$ post; population weights; state-clustered standard errors), estimated on the sample indicated. Under the preferred 90_137 threshold, the point estimate remains positive across all samples; when Rio de Janeiro is excluded the first-stage F drops and Anderson-Rubin confidence sets widen, but the AR set continues to exclude zero for vote share. The 90_120 alternative produces a tighter first stage but smaller point estimate. The Anderson-Rubin 95% confidence set is the closure of the b0-grid set $\{b : p_{\text{AR}}(b) \geq 0.05\}$ from b0 = -15 to +25 in steps of 0.05; it is robust to arbitrary instrument weakness. The 90_137 specification is adopted as preferred (as of 2026-05-26).

Table C.18: Implicit-Weight Decomposition of the 2SLS Estimator

	90_137 (preferred)	**90_120** (alternative)
Municipalities	2105	2105
Share with $\omega < 0$	20.3%	4.8%
$\lvert\Sigma\omega^{-}\rvert/\Sigma\omega^{+}$	4.24%	0.39%
Top-1 muni share of $\lvert\omega\rvert$	17.4%	19.9%
Top-5 munis share of $\lvert\omega\rvert$	47.0%	42.0%
Dominant UF (*SP*)	23.8%	27.3%
Rio de Janeiro UF share	19.2%	21.4%
Rio-capital alone share	17.4%	19.9%

Notes: Implicit weights ω_i are computed via Frisch-Waugh-Lovell residualization of the instrument and endogenous variable against the preferred control set and fixed effects (Blandhol, Bonney, Mogstad, and Torgovitsky, 2022; Goldsmith-Pinkham, Hull, and Kolesár, 2022). A 2SLS coefficient is a weighted average of unit-level responses with $\omega_i = \tilde{Z}_i \cdot \tilde{D}_i \cdot \text{pop}_m / \sum_j \tilde{Z}_j \tilde{D}_j \text{pop}_j$. Under Callaway-Goodman-Bacon-Sant'Anna (2024) the ACR-consistent weights are non-negative; the share with $\omega_i < 0$ measures deviation from that benchmark. The 90_137 (preferred) threshold concentrates identifying mass on a smaller set of high-leverage municipalities than the 90_120 alternative.

Table C.19: Alternative Estimators under the 90_137 Instrument (preferred)

Estimator	$\hat{\beta}$	SE
Baseline 2SLS (preferred)	4.989	1.433
Saturated 2SLS (7 instruments)	3.257	1.041
JIVE (Kolesár 2013)	5.039	1.268
CGBS reduced-form ACR (implied)	2.439	—
Anderson-Rubin 95% CI (Baseline 2SLS)	[2.35, 8.45]	
Sargan J (saturated, df=6)	86.69	$p = 0.000$
CGBS reduced-form slope	0.548	SE 0.384
CGBS first-stage slope	0.225	—

Notes: All estimators are computed under the preferred 90_137 instrument. The baseline 2SLS reproduces Section 4. The saturated 2SLS adds interactions of the instrument with the six baseline characteristics as additional instruments, in the spirit of Blandhol, Bonney, Mogstad and Torgovitsky (2022): under their LATE-recovery argument (formally established for binary treatment with covariates and extended here to our continuous setting), the saturated specification addresses covariate-conditional heterogeneity. A small Sargan J p-value indicates heterogeneity across covariate cells, consistent with the average-causal-response framework (Angrist and Imbens, 1995) we adopt. JIVE follows Kolesár (2013) and uses leverage-adjusted leave-one-out fitted values in the first stage, eliminating the own-observation bias of 2SLS. The CGBS reduced form implements a dCDH-Vazquez-Bare (2024) / Callaway-Goodman-Bacon-Sant'Anna (2024) ACR at the municipality level, regressing first-differenced outcome on the quota shock (state fixed effects, population weights); the implied 2SLS is the ratio of the CGBS reduced form to the CGBS first stage. Standard errors are cluster-robust at the state level where applicable.

Table C.20: First-Stage Estimates (Post-Interacted Endogenous Specification)

	Realized Expansion in 2009		
	(1)	(2)	(3)
ΔQuota_m (π_1)	1.287	1.146	0.615
	(0.515)	(0.287)	(0.114)
Party Fixed Effects		✓	✓
Baseline Characteristics			✓
Observations	4,210	4,210	4,210

Notes: Each coefficient is estimated on the municipality-year panel of 2008 and 2012 elections for the 2,105 municipalities where the 2008 incumbent ran for reelection in 2012. All specifications include year and municipality fixed effects. "Party Fixed Effects" are based on the pre-reform winner's party. "Baseline Characteristics" bundles five Census 2000 controls, each interacted with the post-reform indicator: log population, share of poor, household income per capita, urban share, and the 2000–2006 population growth factor that enters the counterfactual quota formula. All regressions are weighted by municipal population in 2000. Robust standard errors clustered at the state level (26 clusters) are reported in parentheses.

Table C.21: Reduced-Form Estimates (Post-Interacted Endogenous Specification)

	Vote Share			Winning		
	(1)	(2)	(3)	(4)	(5)	(6)
ΔQuota_m (ρ_1)	0.949	1.209	0.986	1.819	2.567	1.133
	(0.198)	(0.269)	(0.238)	(0.703)	(0.476)	(0.385)
Party Fixed Effects		✓	✓		✓	✓
Baseline Characteristics			✓			✓
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: Each coefficient is estimated on the municipality-year panel of 2008 and 2012 elections for the 2,105 municipalities where the 2008 incumbent ran for reelection in 2012. All specifications include year and municipality fixed effects. "Party Fixed Effects" are based on the pre-reform winner's party. "Baseline Characteristics" bundles five Census 2000 controls, each interacted with the post-reform indicator: log population, share of poor, household income per capita, urban share, and the 2000–2006 population growth factor that enters the counterfactual quota formula. All regressions are weighted by municipal population in 2000. Robust standard errors clustered at the state level (26 clusters) are reported in parentheses.

Table C.22: Two-Stage Least Squares Estimates (Post-Interacted Endogenous Specification)

	Vote Share			Winning		
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	0.738	1.055	1.603	1.413	2.241	1.842
	(0.249)	(0.277)	(0.540)	(0.273)	(0.346)	(0.616)
Kleibergen-Paap F	6.24	15.91	29.36	6.24	15.91	29.36
AR 95% CI	[0.450, 3.300]	[0.600, 2.000]	[0.750, 3.200]	[0.800, 2.950]	[1.700, 3.450]	[0.650, 3.350]
Party Fixed Effects		✓	✓		✓	✓
Baseline Characteristics			✓			✓
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: Each coefficient is estimated on the municipality-year panel of 2008 and 2012 elections for the 2,105 municipalities where the 2008 incumbent ran for reelection in 2012. All specifications include year and municipality fixed effects. "Party Fixed Effects" are based on the pre-reform winner's party. "Baseline Characteristics" bundles five Census 2000 controls, each interacted with the post-reform indicator: log population, share of poor, household income per capita, urban share, and the 2000–2006 population growth factor that enters the counterfactual quota formula. All regressions are weighted by municipal population in 2000. Robust standard errors clustered at the state level (26 clusters) are reported in parentheses. The Kleibergen-Paap F-statistic is the squared t-ratio of the quota shock in the first-stage regression with cluster-robust standard errors. Anderson-Rubin 95% confidence sets are robust to arbitrary instrument weakness.

Table C.23: Robustness Checks: Vote Share (Post-Interacted Endogenous Specification)

	Main	Lag VS 2008	Ext. Window	Winsorized	Alt. Denominator	IPW
Panel A: First Stage						
ΔQuota_m (π_1)	0.615	0.614	0.756	0.615	0.615	0.649
	(0.114)	(0.113)	(0.131)	(0.114)	(0.114)	(0.101)
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	0.986	0.880	0.986	0.875	0.913	0.921
	(0.238)	(0.198)	(0.238)	(0.215)	(0.223)	(0.235)
Panel C: Two-Stage Least Squares						
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	1.603	1.432	1.304	1.423	1.484	1.421
	(0.540)	(0.474)	(0.388)	(0.480)	(0.493)	(0.497)
Kleibergen-Paap F	29.36	29.28	33.35	29.36	29.36	41.30
AR 95% CI	[0.750, 3.200]	[0.700, 2.850]	[0.650, 2.350]	[0.650, 2.800]	[0.700, 2.900]	[0.600, 2.800]
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: All columns report the preferred specification: party fixed effects + baseline characteristics $\times$ post, population-2000 weights. The outcome is incumbent vote share except where noted. Column (2) adds the 2008 incumbent vote share $\times$ post to the baseline-characteristics bundle, testing sensitivity to including a lagged dependent variable (Nickell, 1981; Imai and Kim, 2019); the main specification omits this term. Column (3) uses the 12-month moving average of realized beneficiaries through December 2012 as the endogenous variable. Column (4) winsorizes the outcome at the 1st and 99th percentiles. Column (5) uses total votes cast (including blank and null) as the denominator. Column (6) weights by the inverse probability of selection into the rerunning sample. Anderson-Rubin 95% confidence sets are robust to arbitrary instrument weakness. Robust standard errors clustered at the state level in parentheses.

Table C.24: Plausibly Exogenous Bounded-Direct-Effect Sensitivity

	Incumbent's Vote Share		
	(1)	(2)	(3)
ΔQuota_m (ρ_1)	1.929	1.299	1.414
	(0.604)†	(0.461)†	(0.482)†
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	6.082	3.619	4.690
	(2.322)	(1.569)	(1.991)
Strict AR 95% CI ($\gamma = 0$)	[2.019, 12.834]	[0.876, 7.978]	[1.242, 10.282]
$\lvert\gamma\rvert \leq 10\%\lvert\widehat{\rho}_1\rvert$: excludes zero	✓	✓	✓
$\lvert\gamma\rvert \leq 25\%\lvert\widehat{\rho}_1\rvert$: excludes zero	✓	✓	✓
$\lvert\gamma\rvert \leq 50\%\lvert\widehat{\rho}_1\rvert$: excludes zero			
Direct-effect breakdown (% of $\lvert\widehat{\rho}_1\rvert$)	35.5	26.9	29.8
Incumbent Party	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓
Municipality Characteristics		✓	✓
Mayor Characteristics		✓	✓
Benefit Value			✓
FPM Transfers			✓
Number of Mayors / Municipalities	2,078	2,078	2,078
Observations	4,156	4,156	4,156

Notes: The table allows a bounded direct effect γ of the quota shock. Check marks indicate that zero remains outside the 95% union-of-AR set at the stated symmetric bound; the breakdown is the smallest such share at which zero enters. † denotes the wild-cluster result for the reduced form.

Table C.25: Additional Exclusion-Restriction Sensitivity

	Incumbent's Vote Share		
	(1)	(2)	(3)
Panel A: Cinelli–Hazlett Robustness Values			
Reduced-form RV (% partial R^2)	20.21	14.08	16.01
First-stage RV (% partial R^2)	44.46	51.88	51.62
Weakest-link RV: RF or first stage (% partial R^2)	20.21	14.08	16.01
Panel B: Nevo–Rosen Imperfect-Instrument Bounds			
Standard IV coefficient	6.082	3.619	4.690
Synthetic-instrument IV coefficient	-3.931	-3.700	-3.559
$\text{Cov}(D, U) > 0$: upper bound	-3.931	-3.700	-3.559
$\text{Cov}(D, U) < 0$: lower bound	6.082	3.619	4.690
Panel C: Masten–Poirier Falsification-Adaptive Set			
Candidate quota formulas retained ($F \geq 10$)	3	2	2
Minimum retained own-instrument F	10.20	15.65	16.66
Falsification-adaptive set	[-2.965, 1.076]	[-4.605, -3.218]	[-4.974, -3.412]
Incumbent Party	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓
Municipality Characteristics		✓	✓
Mayor Characteristics		✓	✓
Benefit Value			✓
FPM Transfers			✓

Notes: Panel A adapts Cinelli–Hazlett robustness values to state-clustered t statistics with $G - 1$ degrees of freedom. The weakest-link row is the smaller reduced-form or first-stage value; it is a heuristic diagnostic, not a formal IV robustness value. Panel B reports Nevo–Rosen one-sided point bounds under same-sign and relative-correlation restrictions; the sign of $\text{Cov}(D, U)$ is stated explicitly. Panel C uses the three R$90, R$90/120, and R$90/137 quota-formula instruments, controls for the other candidates, screens on conditional own-instrument $F \geq 10$, and reports the Masten–Poirier falsification-adaptive interval.

Table C.26: Close-Election Subsamples, Unweighted Raw Beneficiary-Family Levels: Vote Share

	12-month design through 2009							12-month design through 2012				
	Full (1)	$\leq$ 20 pp (2)	$\leq$ 10 pp (3)	$\leq$ 5 pp (4)	$\leq$ 2.5 pp (5)	Full + Quota 2012 (6)	Full + Δ Quota (7)	Full (8)	$\leq$ 20 pp (9)	$\leq$ 10 pp (10)	$\leq$ 5 pp (11)	$\leq$ 2.5 pp (12)
Panel A: First Stage												
ΔQuota_m (π_1)	0.362	0.716	0.861	1.133	1.838	0.155	0.207	0.545	0.580	0.622	0.629	0.631
	(0.290)	(0.355)	(0.540)	(0.622)	(0.516)	(0.051)	(0.121)	(0.080)	(0.057)	(0.012)	(0.008)	(0.007)
Panel B: Reduced Form												
ΔQuota_m (ρ_1)	0.669	0.641	0.741	0.659	0.968	0.647	0.605	0.177	0.131	0.111	0.102	0.133
	(0.351)	(0.271)	(0.533)	(0.388)	(0.441)	(0.381)	(0.378)	(0.056)	(0.045)	(0.038)	(0.030)	(0.034)
RF wild-cluster p-value	0.100	0.058	0.199	0.232	0.287	0.138	0.160	0.199	0.114	0.295	0.278	0.191
Panel C: Two-Stage Least Squares												
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	1.845	0.895	0.861	0.582	0.527	4.174	2.925	0.326	0.226	0.179	0.163	0.211
	(1.568)	(0.488)	(0.800)	(0.427)	(0.204)	(2.657)	(2.308)	(0.144)	(0.096)	(0.062)	(0.048)	(0.055)
Kleibergen–Paap F	1.56	4.07	2.54	3.32	12.67	9.35	2.91	45.79	102.57	2770.65	6556.03	8906.45
AR 95% CI	$[-\infty, +\infty]$	$[-\infty, +\infty]$	$[-\infty, +\infty]$	$[-\infty, +\infty]$	[0.04, 1.08]	[-1.02, 14.73]	$[-\infty, +\infty]$	[0.09, 0.74]	[0.06, 0.46]	[0.05, 0.31]	[0.06, 0.26]	[0.10, 0.33]
Municipalities	2,078	1,684	1,085	595	291	2,078	2,078	2,078	1,684	1,085	595	291
Observations	4,156	3,368	2,170	1,190	582	4,156	4,156	4,156	3,368	2,170	1,190	582
State Clusters	26	26	26	26	25	26	26	26	26	26	26	25

Notes: Samples are nested using the absolute 2008 decisive-round mayoral victory margin in percentage points; the full column imposes no margin restriction. The margin is measured before the 2009 quota reform and is complete for the common sample. No municipality is removed because of its state's representation after applying the margin cutoff. This is a close-election subsample analysis, not a regression-discontinuity design: the Bolsa Família quota shock is not assigned by crossing an electoral-margin cutoff. Every column uses this paper's Project Full Column (6): 2008 winning-party-by-post fixed effects, realized 2008 BF intensity, municipality characteristics, mayor characteristics, and municipality and year fixed effects. It does not use Gerard–Naritomi–Silva controls, population trimming, state-by-post fixed effects, or the post-treatment exclusion-channel correction. The final two columns in the 2009 group retain the full sample and Project Full specification but additionally control, one at a time, for the authorized 2012 quota or the authorized 2012–2009 quota change interacted with post. The controls follow the table's raw or per-10,000 scale. They are forward-looking robustness checks rather than baseline covariates or heterogeneity estimates, and no analogous columns are reported for the 2012 treatment because those quantities define or coincide with its horizon. Every regression is unweighted. The instrument and endogenous variable are each divided by 1,000. Thus π_1 is the change in thousands of realized beneficiary families per one-thousand-family quota shock; ρ_1 is the change in incumbent vote-share percentage points per one-thousand-family quota shock; and β_1 is the change in vote-share percentage points per 1,000 additional realized beneficiary families. Reduced-form p-values use the wild-cluster bootstrap by state; blue reduced-form cells use these bootstrap p-values. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero. Blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.27: Close-Election Subsamples, Population-Weighted Raw Levels: Vote Share

	12-month design through 2009							12-month design through 2012				
	Full (1)	$\leq$ 20 pp (2)	$\leq$ 10 pp (3)	$\leq$ 5 pp (4)	$\leq$ 2.5 pp (5)	Full + Quota 2012 (6)	Full + Δ Quota (7)	Full (8)	$\leq$ 20 pp (9)	$\leq$ 10 pp (10)	$\leq$ 5 pp (11)	$\leq$ 2.5 pp (12)
Panel A: First Stage												
ΔQuota_m (π_1)	1.049	1.488	1.641	1.562	2.372	0.245	0.365	0.570	0.604	0.615	0.619	0.619
	(0.183)	(0.221)	(0.186)	(0.299)	(0.164)	(0.070)	(0.095)	(0.030)	(0.018)	(0.098)	(0.007)	(0.007)
Panel B: Reduced Form												
ΔQuota_m (ρ_1)	0.964	0.550	0.187	0.228	0.210	0.900	0.731	0.153	0.053	-0.006	0.009	0.053
	(0.385)	(0.246)	(0.420)	(0.462)	(0.541)	(0.463)	(0.431)	(0.062)	(0.036)	(0.057)	(0.062)	(0.080)
RF wild-cluster p-value	0.068	0.161	0.742	0.651	0.746	0.160	0.218	0.064	0.234	0.932	0.902	0.602
Panel C: Two-Stage Least Squares												
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	0.919	0.369	0.114	0.146	0.089	3.666	2.003	0.269	0.088	-0.010	0.015	0.086
	(0.385)	(0.194)	(0.289)	(0.387)	(0.229)	(1.700)	(1.228)	(0.130)	(0.060)	(0.090)	(0.111)	(0.129)
Kleibergen–Paap F	32.72	45.23	77.49	27.33	209.63	12.35	14.88	370.32	1163.68	39.83	8885.88	8015.73
AR 95% CI	[0.17, 1.79]	[0.02, 0.90]	[-0.35, 0.79]	[-0.93, 0.44]	[-0.38, 0.58]	[-0.20, 12.18]	[-0.38, 6.98]	[0.04, 0.51]	[-0.03, 0.22]	[-0.20, 0.21]	[-0.19, 0.22]	[-0.18, 0.35]
Municipalities	2,078	1,684	1,085	595	291	2,078	2,078	2,078	1,684	1,085	595	291
Observations	4,156	3,368	2,170	1,190	582	4,156	4,156	4,156	3,368	2,170	1,190	582
State Clusters	26	26	26	26	25	26	26	26	26	26	26	25

Notes: Samples are nested using the absolute 2008 decisive-round mayoral victory margin in percentage points; the full column imposes no margin restriction. The margin is measured before the 2009 quota reform and is complete for the common sample. No municipality is removed because of its state's representation after applying the margin cutoff. This is a close-election subsample analysis, not a regression-discontinuity design: the Bolsa Família quota shock is not assigned by crossing an electoral-margin cutoff. Every column uses this paper's Project Full Column (6): 2008 winning-party-by-post fixed effects, realized 2008 BF intensity, municipality characteristics, mayor characteristics, and municipality and year fixed effects. It does not use Gerard–Naritomi–Silva controls, population trimming, state-by-post fixed effects, or the post-treatment exclusion-channel correction. The final two columns in the 2009 group retain the full sample and Project Full specification but additionally control, one at a time, for the authorized 2012 quota or the authorized 2012–2009 quota change interacted with post. The controls follow the table's raw or per-10,000 scale. They are forward-looking robustness checks rather than baseline covariates or heterogeneity estimates, and no analogous columns are reported for the 2012 treatment because those quantities define or coincide with its horizon. Every regression uses 2006-population weights within the selected sample. The instrument and endogenous variable are each divided by 1,000. Thus π_1 is the change in thousands of realized beneficiary families per one-thousand-family quota shock; ρ_1 is the change in incumbent vote-share percentage points per one-thousand-family quota shock; and β_1 is the change in vote-share percentage points per 1,000 additional realized beneficiary families. Reduced-form p-values use the wild-cluster bootstrap by state; blue reduced-form cells use these bootstrap p-values. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero. Blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.28: Close-Election Subsamples, Unweighted Rates per 10,000 Residents: Vote Share

	12-month design through 2009							12-month design through 2012				
	Full (1)	≤ 20 pp (2)	≤ 10 pp (3)	≤ 5 pp (4)	≤ 2.5 pp (5)	Full + Quota 2012 (6)	Full + Δ Quota (7)	Full (8)	≤ 20 pp (9)	≤ 10 pp (10)	≤ 5 pp (11)	≤ 2.5 pp (12)
Panel A: First Stage												
ΔQuota_m (π_1)	0.127	0.127	0.120	0.155	0.180	0.085	0.132	0.471	0.477	0.475	0.573	0.629
	(0.014)	(0.016)	(0.025)	(0.038)	(0.055)	(0.012)	(0.017)	(0.049)	(0.053)	(0.078)	(0.094)	(0.117)
Panel B: Reduced Form												
ΔQuota_m (ρ_1)	-0.001	-0.001	0.001	-0.001	0.001	0.000	-0.001	-0.001	0.000	0.002	0.000	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
RF wild-cluster p-value	0.443	0.616	0.723	0.682	0.607	0.789	0.455	0.632	0.814	0.289	0.822	0.552
Panel C: Two-Stage Least Squares												
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-0.009	-0.004	0.005	-0.006	0.007	-0.005	-0.009	-0.002	0.001	0.003	0.001	-0.002
	(0.011)	(0.008)	(0.015)	(0.014)	(0.013)	(0.018)	(0.011)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
Kleibergen–Paap F	79.29	63.94	23.00	16.99	10.78	49.63	62.79	91.06	80.77	37.05	36.78	29.01
AR 95% CI	[-0.033, 0.015]	[-0.021, 0.014]	[-0.027, 0.041]	[-0.053, 0.019]	[-0.033, 0.037]	[-0.046, 0.032]	[-0.033, 0.016]	[-0.008, 0.005]	[-0.005, 0.007]	[-0.003, 0.011]	[-0.005, 0.005]	[-0.010, 0.003]
Municipalities	2,078	1,684	1,085	595	291	2,078	2,078	2,078	1,684	1,085	595	291
Observations	4,156	3,368	2,170	1,190	582	4,156	4,156	4,156	3,368	2,170	1,190	582
State Clusters	26	26	26	26	25	26	26	26	26	26	26	25

Notes: Samples are nested using the absolute 2008 decisive-round mayoral victory margin in percentage points; the full column imposes no margin restriction. The margin is measured before the 2009 quota reform and is complete for the common sample. No municipality is removed because of its state's representation after applying the margin cutoff. This is a close-election subsample analysis, not a regression-discontinuity design: the Bolsa Família quota shock is not assigned by crossing an electoral-margin cutoff. Every column uses this paper's Project Full Column (6): 2008 winning-party-by-post fixed effects, realized 2008 BF intensity, municipality characteristics, mayor characteristics, and municipality and year fixed effects. It does not use Gerard–Naritomi–Silva controls, population trimming, state-by-post fixed effects, or the post-treatment exclusion-channel correction. The final two columns in the 2009 group retain the full sample and Project Full specification but additionally control, one at a time, for the authorized 2012 quota or the authorized 2012–2009 quota change interacted with post. The controls follow the table's raw or per-10,000 scale. They are forward-looking robustness checks rather than baseline covariates or heterogeneity estimates, and no analogous columns are reported for the 2012 treatment because those quantities define or coincide with its horizon. Every regression is unweighted. The instrument and endogenous variable are each divided by 2006 population and multiplied by 10,000. Thus π_1 is the change in realized families per 10,000 residents per one-unit quota-rate shock; ρ_1 is the change in incumbent vote-share percentage points per one-unit quota-rate shock; and β_1 is the change in vote-share percentage points per one additional realized beneficiary family per 10,000 residents. Reduced-form p-values use the wild-cluster bootstrap by state; blue reduced-form cells use these bootstrap p-values. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero. Blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.29: Close-Election Subsamples, Population-Weighted Rates per 10,000: Vote Share

	12-month design through 2009							12-month design through 2012				
	Full (1)	≤ 20 pp (2)	≤ 10 pp (3)	≤ 5 pp (4)	≤ 2.5 pp (5)	Full + Quota 2012 (6)	Full + Δ Quota (7)	Full (8)	≤ 20 pp (9)	≤ 10 pp (10)	≤ 5 pp (11)	≤ 2.5 pp (12)
Panel A: First Stage												
ΔQuota_m (π_1)	0.111	0.126	0.121	0.146	0.145	0.078	0.104	0.380	0.430	0.461	0.546	0.598
	(0.019)	(0.022)	(0.020)	(0.026)	(0.044)	(0.018)	(0.025)	(0.066)	(0.064)	(0.071)	(0.077)	(0.109)
Panel B: Reduced Form												
ΔQuota_m (ρ_1)	0.004	0.001	0.001	-0.001	0.001	0.005	0.007	0.008	0.003	0.000	0.000	-0.001
	(0.004)	(0.002)	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
RF wild-cluster p-value	0.418	0.662	0.911	0.887	0.735	0.361	0.214	0.024	0.236	0.991	0.891	0.562
Panel C: Two-Stage Least Squares												
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	0.037	0.008	0.004	-0.004	0.006	0.066	0.065	0.020	0.007	0.000	0.001	-0.002
	(0.040)	(0.016)	(0.034)	(0.035)	(0.019)	(0.054)	(0.042)	(0.009)	(0.005)	(0.006)	(0.005)	(0.004)
Kleibergen–Paap F	34.65	33.03	38.10	31.52	10.71	17.68	17.51	33.13	45.79	42.50	49.72	29.90
AR 95% CI	[-0.038, 0.136]	[-0.026, 0.044]	[-0.065, 0.083]	[-0.088, 0.057]	[-0.051, 0.048]	[-0.051, 0.212]	[-0.014, 0.201]	[0.005, 0.045]	[-0.004, 0.016]	[-0.015, 0.013]	[-0.014, 0.012]	[-0.010, 0.006]
Municipalities	2,078	1,684	1,085	595	291	2,078	2,078	2,078	1,684	1,085	595	291
Observations	4,156	3,368	2,170	1,190	582	4,156	4,156	4,156	3,368	2,170	1,190	582
State Clusters	26	26	26	26	25	26	26	26	26	26	26	25

Notes: Samples are nested using the absolute 2008 decisive-round mayoral victory margin in percentage points; the full column imposes no margin restriction. The margin is measured before the 2009 quota reform and is complete for the common sample. No municipality is removed because of its state's representation after applying the margin cutoff. This is a close-election subsample analysis, not a regression-discontinuity design: the Bolsa Família quota shock is not assigned by crossing an electoral-margin cutoff. Every column uses this paper's Project Full Column (6): 2008 winning-party-by-post fixed effects, realized 2008 BF intensity, municipality characteristics, mayor characteristics, and municipality and year fixed effects. It does not use Gerard–Naritomi–Silva controls, population trimming, state-by-post fixed effects, or the post-treatment exclusion-channel correction. The final two columns in the 2009 group retain the full sample and Project Full specification but additionally control, one at a time, for the authorized 2012 quota or the authorized 2012–2009 quota change interacted with post. The controls follow the table's raw or per-10,000 scale. They are forward-looking robustness checks rather than baseline covariates or heterogeneity estimates, and no analogous columns are reported for the 2012 treatment because those quantities define or coincide with its horizon. Every regression uses 2006-population weights within the selected sample. The instrument and endogenous variable are each divided by 2006 population and multiplied by 10,000. Thus π_1 is the change in realized families per 10,000 residents per one-unit quota-rate shock; ρ_1 is the change in incumbent vote-share percentage points per one-unit quota-rate shock; and β_1 is the change in vote-share percentage points per one additional realized beneficiary family per 10,000 residents. Reduced-form p-values use the wild-cluster bootstrap by state; blue reduced-form cells use these bootstrap p-values. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero. Blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.30: Discrete Quota-Shock Instruments, Unweighted Raw Levels: Vote Share

	12-month design through 2009				12-month design through 2012			
	Continuous (1)	$Z \geq P25$ (2)	$Z \geq P50$ (3)	$Z \geq P75$ (4)	Continuous (5)	$Z \geq P25$ (6)	$Z \geq P50$ (7)	$Z \geq P75$ (8)
Panel A: First Stage								
Instrument × Post (π_1)	0.380	0.266	0.108	0.012	0.556	0.315	0.095	0.030
	(0.298)	(0.098)	(0.043)	(0.045)	(0.074)	(0.121)	(0.071)	(0.128)
Panel B: Reduced Form								
Instrument × Post (ρ_1)	0.776	1.034	-1.142	-0.639	0.228	0.854	0.936	0.704
	(0.342)	(0.906)	(0.573)	(0.667)	(0.060)	(0.924)	(0.583)	(0.693)
RF wild-cluster p-value	0.048	0.250	0.063	0.361	0.442	0.384	0.125	0.350
Panel C: Two-Stage Least Squares								
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	2.043	3.893	-10.602	-52.611	0.409	2.708	9.874	23.369
	(1.421)	(3.592)	(8.414)	(197.701)	(0.155)	(2.739)	(7.505)	(97.575)
Kleibergen–Paap F	1.63	7.40	6.21	0.07	56.34	6.82	1.80	0.06
AR 95% CI	$[-\infty, +\infty]$	[-3.97, 20.19]	[-97.82, 0.24]	$[-\infty, +\infty]$	[0.15, 0.84]	[-6.47, 11.89]	$[-\infty, +\infty]$	$[-\infty, +\infty]$
Shock cutoff	—	-0.124	0.022	0.141	—	-0.067	0.028	0.193
$Z = 1$ municipalities	—	1,558	1,039	520	—	1,558	1,039	520
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
State × Post Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Complete Predetermined Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipalities	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156
State Clusters	26	26	26	26	26	26	26	26

Notes: The first column in each horizon uses the continuous quota shock. The remaining columns replace it with an indicator that the continuous shock is at or above the unweighted P25, P50, or P75 of the common 2,078-municipality sample; the endogenous BF expansion remains continuous in every regression. Cutoffs are calculated separately by horizon and scale and then held fixed across weighted and unweighted tables. There are no ties at any displayed cutoff. The binary columns are alternative instruments, not split-sample estimates. Using the complementary below-cutoff indicator would reverse the first-stage and reduced-form signs but leave the 2SLS estimate, AR set, and first-stage F unchanged because post is absorbed by year fixed effects. Thresholding discards continuous first-stage information and can change the IV complier weights, so coefficients need not equal the continuous-instrument estimate. Every column starts from Project Full Column (6) and then adds state-by-post fixed effects as the explicit robustness adjustment in main Column (10). Gerard–Naritomi–Silva trend controls and the post-treatment exclusion-channel correction are omitted. Every regression is unweighted. The continuous shock and continuous endogenous BF expansion are divided by 1,000. Binary first-stage and reduced-form coefficients compare municipalities above and below the displayed shock threshold; every 2SLS coefficient is the change in incumbent vote-share percentage points per 1,000 additional realized beneficiary families. Reduced-form p-values use the wild-cluster bootstrap by state; blue reduced-form cells use these bootstrap p-values. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero. Blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.31: Discrete Quota-Shock Instruments, Population-Weighted Raw Levels: Vote Share

	12-month design through 2009				12-month design through 2012			
	Continuous (1)	$Z \geq P25$ (2)	$Z \geq P50$ (3)	$Z \geq P75$ (4)	Continuous (5)	$Z \geq P25$ (6)	$Z \geq P50$ (7)	$Z \geq P75$ (8)
Panel A: First Stage								
Instrument × Post (π_1)	0.971	2.505	1.552	0.298	0.590	0.616	-0.939	-3.198
	(0.345)	(1.010)	(0.585)	(0.443)	(0.022)	(0.951)	(1.104)	(2.186)
Panel B: Reduced Form								
Instrument × Post (ρ_1)	0.565	1.845	0.440	0.623	0.129	3.420	1.709	1.768
	(0.444)	(2.657)	(2.104)	(2.060)	(0.053)	(1.200)	(1.109)	(1.104)
RF wild-cluster p-value	0.328	0.606	0.862	0.868	0.130	0.017	0.171	0.114
Panel C: Two-Stage Least Squares								
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	0.582	0.737	0.284	2.090	0.219	5.551	-1.819	-0.553
	(0.449)	(0.981)	(1.458)	(8.747)	(0.080)	(9.781)	(2.941)	(0.618)
Kleibergen–Paap F	7.92	6.15	7.04	0.45	721.40	0.42	0.72	2.14
AR 95% CI	[-0.52, 2.27]	[-2.22, 6.13]	[-4.47, 4.34]	$[-\infty, +\infty]$	[0.03, 0.41]	$[-\infty, +\infty]$	$[-\infty, +\infty]$	$[-\infty, +\infty]$
Shock cutoff	—	-0.124	0.022	0.141	—	-0.067	0.028	0.193
$Z = 1$ municipalities	—	1,558	1,039	520	—	1,558	1,039	520
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
State × Post Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Complete Predetermined Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipalities	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156
State Clusters	26	26	26	26	26	26	26	26

Notes: The first column in each horizon uses the continuous quota shock. The remaining columns replace it with an indicator that the continuous shock is at or above the unweighted P25, P50, or P75 of the common 2,078-municipality sample; the endogenous BF expansion remains continuous in every regression. Cutoffs are calculated separately by horizon and scale and then held fixed across weighted and unweighted tables. There are no ties at any displayed cutoff. The binary columns are alternative instruments, not split-sample estimates. Using the complementary below-cutoff indicator would reverse the first-stage and reduced-form signs but leave the 2SLS estimate, AR set, and first-stage F unchanged because post is absorbed by year fixed effects. Thresholding discards continuous first-stage information and can change the IV complier weights, so coefficients need not equal the continuous-instrument estimate. Every column starts from Project Full Column (6) and then adds state-by-post fixed effects as the explicit robustness adjustment in main Column (10). Gerard–Naritomi–Silva trend controls and the post-treatment exclusion-channel correction are omitted. Every regression uses 2006-population weights. The continuous shock and continuous endogenous BF expansion are divided by 1,000. Binary first-stage and reduced-form coefficients compare municipalities above and below the displayed shock threshold; every 2SLS coefficient is the change in incumbent vote-share percentage points per 1,000 additional realized beneficiary families. Reduced-form p-values use the wild-cluster bootstrap by state; blue reduced-form cells use these bootstrap p-values. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero. Blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.32: Discrete Quota-Shock Instruments, Unweighted Rates per 10,000: Vote Share

	12-month design through 2009				12-month design through 2012			
	Continuous (1)	$Z \geq P25$ (2)	$Z \geq P50$ (3)	$Z \geq P75$ (4)	Continuous (5)	$Z \geq P25$ (6)	$Z \geq P50$ (7)	$Z \geq P75$ (8)
Panel A: First Stage								
Instrument × Post (π_1)	0.151	39.456	42.022	54.287	0.545	155.131	159.525	201.450
	(0.014)	(5.887)	(5.266)	(5.447)	(0.039)	(16.993)	(16.425)	(15.651)
Panel B: Reduced Form								
Instrument × Post (ρ_1)	-0.001	0.317	-0.555	-1.363	0.001	0.842	0.473	-0.319
	(0.001)	(0.702)	(0.554)	(0.740)	(0.001)	(0.914)	(0.708)	(0.763)
RF wild-cluster p-value	0.682	0.652	0.342	0.071	0.490	0.385	0.524	0.684
Panel C: Two-Stage Least Squares								
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-0.003	0.008	-0.013	-0.025	0.002	0.005	0.003	-0.002
	(0.008)	(0.018)	(0.014)	(0.014)	(0.003)	(0.006)	(0.004)	(0.004)
Kleibergen–Paap F	112.39	44.92	63.68	99.34	199.01	83.34	94.33	165.68
AR 95% CI	[-0.021, 0.014]	[-0.031, 0.045]	[-0.047, 0.013]	[-0.057, 0.003]	[-0.003, 0.007]	[-0.008, 0.017]	[-0.007, 0.011]	[-0.010, 0.006]
Shock cutoff	—	-110.450	27.873	136.565	—	-75.636	28.622	165.420
$Z = 1$ municipalities	—	1,558	1,039	520	—	1,558	1,039	520
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
State × Post Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Complete Predetermined Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipalities	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156
State Clusters	26	26	26	26	26	26	26	26

Notes: The first column in each horizon uses the continuous quota shock. The remaining columns replace it with an indicator that the continuous shock is at or above the unweighted P25, P50, or P75 of the common 2,078-municipality sample; the endogenous BF expansion remains continuous in every regression. Cutoffs are calculated separately by horizon and scale and then held fixed across weighted and unweighted tables. There are no ties at any displayed cutoff. The binary columns are alternative instruments, not split-sample estimates. Using the complementary below-cutoff indicator would reverse the first-stage and reduced-form signs but leave the 2SLS estimate, AR set, and first-stage F unchanged because post is absorbed by year fixed effects. Thresholding discards continuous first-stage information and can change the IV complier weights, so coefficients need not equal the continuous-instrument estimate. Every column starts from Project Full Column (6) and then adds state-by-post fixed effects as the explicit robustness adjustment in main Column (10). Gerard–Naritomi–Silva trend controls and the post-treatment exclusion-channel correction are omitted. Every regression is unweighted. The continuous shock and continuous endogenous BF expansion are divided by 2006 population and multiplied by 10,000. Binary first-stage and reduced-form coefficients compare municipalities above and below the displayed shock-rate threshold; every 2SLS coefficient is the change in incumbent vote-share percentage points per one additional realized beneficiary family per 10,000 residents. Reduced-form p-values use the wild-cluster bootstrap by state; blue reduced-form cells use these bootstrap p-values. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero. Blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.33: Discrete Quota-Shock Instruments, Population-Weighted Rates per 10,000: Vote Share

	12-month design through 2009				12-month design through 2012			
	Continuous (1)	$Z \geq P25$ (2)	$Z \geq P50$ (3)	$Z \geq P75$ (4)	Continuous (5)	$Z \geq P25$ (6)	$Z \geq P50$ (7)	$Z \geq P75$ (8)
Panel A: First Stage								
Instrument × Post (π_1)	0.153	43.998	30.428	47.315	0.557	113.008	129.667	146.652
	(0.014)	(4.573)	(5.246)	(9.391)	(0.041)	(18.779)	(17.349)	(26.439)
Panel B: Reduced Form								
Instrument × Post (ρ_1)	0.000	3.397	-0.479	-1.167	0.004	1.871	0.281	0.404
	(0.003)	(1.920)	(1.637)	(1.281)	(0.002)	(1.305)	(1.534)	(1.566)
RF wild-cluster p-value	0.923	0.214	0.851	0.464	0.032	0.263	0.891	0.821
Panel C: Two-Stage Least Squares								
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	0.003	0.077	-0.016	-0.025	0.006	0.017	0.002	0.003
	(0.024)	(0.042)	(0.055)	(0.029)	(0.003)	(0.010)	(0.011)	(0.010)
Kleibergen–Paap F	115.23	92.58	33.64	25.38	184.46	36.21	55.86	30.77
AR 95% CI	[-0.045, 0.050]	[-0.013, 0.172]	[-0.125, 0.110]	[-0.097, 0.031]	[0.001, 0.012]	[-0.010, 0.034]	[-0.027, 0.024]	[-0.024, 0.023]
Shock cutoff	—	-110.450	27.873	136.565	—	-75.636	28.622	165.420
$Z = 1$ municipalities	—	1,558	1,039	520	—	1,558	1,039	520
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
State × Post Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Complete Predetermined Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipalities	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156
State Clusters	26	26	26	26	26	26	26	26

Notes: The first column in each horizon uses the continuous quota shock. The remaining columns replace it with an indicator that the continuous shock is at or above the unweighted P25, P50, or P75 of the common 2,078-municipality sample; the endogenous BF expansion remains continuous in every regression. Cutoffs are calculated separately by horizon and scale and then held fixed across weighted and unweighted tables. There are no ties at any displayed cutoff. The binary columns are alternative instruments, not split-sample estimates. Using the complementary below-cutoff indicator would reverse the first-stage and reduced-form signs but leave the 2SLS estimate, AR set, and first-stage F unchanged because post is absorbed by year fixed effects. Thresholding discards continuous first-stage information and can change the IV complier weights, so coefficients need not equal the continuous-instrument estimate. Every column starts from Project Full Column (6) and then adds state-by-post fixed effects as the explicit robustness adjustment in main Column (10). Gerard–Naritomi–Silva trend controls and the post-treatment exclusion-channel correction are omitted. Every regression uses 2006-population weights. The continuous shock and continuous endogenous BF expansion are divided by 2006 population and multiplied by 10,000. Binary first-stage and reduced-form coefficients compare municipalities above and below the displayed shock-rate threshold; every 2SLS coefficient is the change in incumbent vote-share percentage points per one additional realized beneficiary family per 10,000 residents. Reduced-form p-values use the wild-cluster bootstrap by state; blue reduced-form cells use these bootstrap p-values. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero. Blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.34: Binary Realized-Expansion Margins, Unweighted Raw Levels: Vote Share

	12-month design through 2009						12-month design through 2012					
	Continuous (1)	$1[\Delta D > 0]$ (2)	$1[\Delta D \geq P25]$ (3)	$1[\Delta D \geq P50]$ (4)	$1[\Delta D \geq P75]$ (5)	$1[\Delta D \geq P90]$ (6)	Continuous (7)	$1[\Delta D > 0]$ (8)	$1[\Delta D \geq P25]$ (9)	$1[\Delta D \geq P50]$ (10)	$1[\Delta D \geq P75]$ (11)	$1[\Delta D \geq P90]$ (12)
Panel A: First Stage												
ΔQuota_m (π_1)	0.362	0.036	0.028	0.044	0.031	0.029	0.545	0.004	0.003	0.001	0.000	0.002
	(0.290)	(0.011)	(0.010)	(0.014)	(0.013)	(0.016)	(0.080)	(0.003)	(0.003)	(0.005)	(0.004)	(0.003)
Panel B: Reduced Form												
ΔQuota_m (ρ_1)	0.669	0.669	0.669	0.669	0.669	0.669	0.177	0.177	0.177	0.177	0.177	0.177
	(0.351)	(0.351)	(0.351)	(0.351)	(0.351)	(0.351)	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)
RF wild-cluster p-value	0.099	0.097	0.098	0.098	0.096	0.099	0.202	0.196	0.199	0.188	0.199	0.200
Panel C: Two-Stage Least Squares												
$\widehat{D}_m$ (β_1)	1.845	18.347	23.485	15.092	21.602	23.353	0.326	40.857	68.434	328.748	670.045	72.822
	(1.568)	(10.890)	(14.911)	(9.281)	(15.098)	(18.267)	(0.144)	(19.469)	(74.166)	(2957.834)	(9420.732)	(76.105)
Kleibergen–Paap F	1.56	11.59	8.01	9.98	6.02	3.33	45.79	2.07	0.56	0.01	0.00	0.77
AR 95% CI	$[-\infty, +\infty]$	[-1.51, 58.30]	[-1.90, 102.43]	[-1.22, 54.04]	[-1.62, 162.51]	$[-\infty, +\infty]$	[0.09, 0.74]	$[-\infty, +\infty]$	$[-\infty, +\infty]$	$[-\infty, +\infty]$	$[-\infty, +\infty]$	$[-\infty, +\infty]$
Municipalities	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156
State Clusters	26	26	26	26	26	26	26	26	26	26	26	26

Notes: The continuous column reproduces this paper's Project Full Column (6). The other columns replace the endogenous BF level with a post-period indicator for any realized increase or an increase at or above the unweighted P25, P50, P75, or P90 of the common municipality sample. The excluded quota shock remains continuous in every column. Thresholds are computed separately by horizon and scale, then held fixed across weighted and unweighted tables. The binary coefficients are vote-share percentage-point effects of an instrument-induced change from zero to one in the probability of crossing the displayed realized-expansion margin; they are not unit conversions of the continuous coefficient and need not identify the same complier-weighted effect. The thresholds use realized post-reform treatment changes and therefore define diagnostic estimands rather than predetermined subgroups. Every column uses Project Full: municipality and year fixed effects, 2008 winning-party-by-post fixed effects, realized 2008 BF intensity, municipality characteristics, and mayor characteristics. State-by-post fixed effects, Gerard–Naritomi–Silva controls, and the post-treatment exclusion-channel correction are omitted. Every regression is unweighted. The instrument and endogenous variable are each divided by 1,000. Thus π_1 is the change in thousands of realized beneficiary families per one-thousand-family quota shock; ρ_1 is the change in incumbent vote-share percentage points per one-thousand-family quota shock; and β_1 is the change in vote-share percentage points per 1,000 additional realized beneficiary families. Reduced-form p-values use the wild-cluster bootstrap by state. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero; blue reduced-form cells use the wild-cluster p-value, and blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.35: Binary Realized-Expansion Margins, Population-Weighted Raw Levels: Vote Share

	12-month design through 2009						12-month design through 2012					
	Continuous (1)	$1[\Delta D > 0]$ (2)	$1[\Delta D \geq P25]$ (3)	$1[\Delta D \geq P50]$ (4)	$1[\Delta D \geq P75]$ (5)	$1[\Delta D \geq P90]$ (6)	Continuous (7)	$1[\Delta D > 0]$ (8)	$1[\Delta D \geq P25]$ (9)	$1[\Delta D \geq P50]$ (10)	$1[\Delta D \geq P75]$ (11)	$1[\Delta D \geq P90]$ (12)
Panel A: First Stage												
ΔQuota_m (π_1)	1.049	0.007	0.006	0.003	-0.003	-0.007	0.570	0.000	0.000	-0.002	-0.004	-0.004
	(0.183)	(0.004)	(0.004)	(0.005)	(0.007)	(0.007)	(0.030)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Panel B: Reduced Form												
ΔQuota_m (ρ_1)	0.964	0.964	0.964	0.964	0.964	0.964	0.153	0.153	0.153	0.153	0.153	0.153
	(0.385)	(0.385)	(0.385)	(0.385)	(0.385)	(0.385)	(0.062)	(0.062)	(0.062)	(0.062)	(0.062)	(0.062)
RF wild-cluster p-value	0.068	0.067	0.067	0.067	0.069	0.068	0.060	0.059	0.060	0.058	0.058	0.060
Panel C: Two-Stage Least Squares												
$\widehat{D}_m$ (β_1)	0.919	141.624	162.769	364.126	-338.658	-146.329	0.269	414.341	-610.462	-69.063	-40.474	-40.603
	(0.385)	(104.634)	(126.842)	(717.289)	(899.808)	(192.925)	(0.130)	(428.949)	(838.741)	(34.310)	(20.054)	(19.870)
Kleibergen–Paap F	32.72	2.48	2.44	0.28	0.17	0.91	370.32	0.97	0.69	26.81	41.56	32.23
AR 95% CI	[0.17, 1.79]	$[-\infty, +\infty]$	$[-\infty, +\infty]$	$[-\infty, +\infty]$	$[-\infty, +\infty]$	$[-\infty, +\infty]$	[0.04, 0.51]	$[-\infty, +\infty]$	$[-\infty, +\infty]$	[-159.80, -10.80]	[-64.65, -8.60]	[-170.06, -3.40]
Municipalities	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156
State Clusters	26	26	26	26	26	26	26	26	26	26	26	26

Notes: The continuous column reproduces this paper's Project Full Column (6). The other columns replace the endogenous BF level with a post-period indicator for any realized increase or an increase at or above the unweighted P25, P50, P75, or P90 of the common municipality sample. The excluded quota shock remains continuous in every column. Thresholds are computed separately by horizon and scale, then held fixed across weighted and unweighted tables. The binary coefficients are vote-share percentage-point effects of an instrument-induced change from zero to one in the probability of crossing the displayed realized-expansion margin; they are not unit conversions of the continuous coefficient and need not identify the same complier-weighted effect. The thresholds use realized post-reform treatment changes and therefore define diagnostic estimands rather than predetermined subgroups. Every column uses Project Full: municipality and year fixed effects, 2008 winning-party-by-post fixed effects, realized 2008 BF intensity, municipality characteristics, and mayor characteristics. State-by-post fixed effects, Gerard–Naritomi–Silva controls, and the post-treatment exclusion-channel correction are omitted. Every regression uses 2006-population weights. The instrument and endogenous variable are each divided by 1,000. Thus π_1 is the change in thousands of realized beneficiary families per one-thousand-family quota shock; ρ_1 is the change in incumbent vote-share percentage points per one-thousand-family quota shock; and β_1 is the change in vote-share percentage points per 1,000 additional realized beneficiary families. Reduced-form p-values use the wild-cluster bootstrap by state. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero; blue reduced-form cells use the wild-cluster p-value, and blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.36: Binary Realized-Expansion Margins, Unweighted Rates per 10,000: Vote Share

	12-month design through 2009						12-month design through 2012					
	Continuous (1)	$1[\Delta D > 0]$ (2)	$1[\Delta D \geq P25]$ (3)	$1[\Delta D \geq P50]$ (4)	$1[\Delta D \geq P75]$ (5)	$1[\Delta D \geq P90]$ (6)	Continuous (7)	$1[\Delta D > 0]$ (8)	$1[\Delta D \geq P25]$ (9)	$1[\Delta D \geq P50]$ (10)	$1[\Delta D \geq P75]$ (11)	$1[\Delta D \geq P90]$ (12)
Panel A: First Stage												
ΔQuota_m (π_1)	0.127	0.000	0.000	0.001	0.001	0.000	0.471	0.000	0.000	0.001	0.001	0.000
	(0.014)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.049)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel B: Reduced Form												
ΔQuota_m (ρ_1)	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
RF wild-cluster p-value	0.445	0.441	0.447	0.433	0.433	0.438	0.626	0.639	0.633	0.631	0.633	0.630
Panel C: Two-Stage Least Squares												
$\widehat{D}_m$ (β_1)	-0.009	-2.875	-3.772	-2.106	-2.160	-3.214	-0.002	-2.420	-2.025	-1.348	-1.302	-1.693
	(0.011)	(3.499)	(4.519)	(2.528)	(2.613)	(3.971)	(0.003)	(4.490)	(3.738)	(2.530)	(2.549)	(3.373)
Kleibergen–Paap F	79.29	73.82	37.36	87.75	168.28	77.06	91.06	25.21	27.58	52.91	61.82	42.25
AR 95% CI	[-0.033, 0.015]	[-9.996, 4.861]	[-12.960, 6.861]	[-7.043, 3.665]	[-7.279, 3.643]	[-11.610, 5.217]	[-0.008, 0.005]	[-11.409, 9.019]	[-9.272, 7.638]	[-6.365, 4.535]	[-7.271, 3.656]	[-10.365, 4.492]
Municipalities	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156
State Clusters	26	26	26	26	26	26	26	26	26	26	26	26

Notes: The continuous column reproduces this paper's Project Full Column (6). The other columns replace the endogenous BF level with a post-period indicator for any realized increase or an increase at or above the unweighted P25, P50, P75, or P90 of the common municipality sample. The excluded quota shock remains continuous in every column. Thresholds are computed separately by horizon and scale, then held fixed across weighted and unweighted tables. The binary coefficients are vote-share percentage-point effects of an instrument-induced change from zero to one in the probability of crossing the displayed realized-expansion margin; they are not unit conversions of the continuous coefficient and need not identify the same complier-weighted effect. The thresholds use realized post-reform treatment changes and therefore define diagnostic estimands rather than predetermined subgroups. Every column uses Project Full: municipality and year fixed effects, 2008 winning-party-by-post fixed effects, realized 2008 BF intensity, municipality characteristics, and mayor characteristics. State-by-post fixed effects, Gerard–Naritomi–Silva controls, and the post-treatment exclusion-channel correction are omitted. Every regression is unweighted. The instrument and endogenous variable are each divided by 2006 population and multiplied by 10,000. Thus π_1 is the change in realized families per 10,000 residents per one-unit quota-rate shock; ρ_1 is the change in incumbent vote-share percentage points per one-unit quota-rate shock; and β_1 is the change in vote-share percentage points per one additional realized beneficiary family per 10,000 residents. Reduced-form p-values use the wild-cluster bootstrap by state. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero; blue reduced-form cells use the wild-cluster p-value, and blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.37: Binary Realized-Expansion Margins, Population-Weighted Rates per 10,000: Vote Share

	12-month design through 2009						12-month design through 2012					
	Continuous (1)	$1[\Delta D > 0]$ (2)	$1[\Delta D \geq P25]$ (3)	$1[\Delta D \geq P50]$ (4)	$1[\Delta D \geq P75]$ (5)	$1[\Delta D \geq P90]$ (6)	Continuous (7)	$1[\Delta D > 0]$ (8)	$1[\Delta D \geq P25]$ (9)	$1[\Delta D \geq P50]$ (10)	$1[\Delta D \geq P75]$ (11)	$1[\Delta D \geq P90]$ (12)
Panel A: First Stage												
ΔQuota_m (π_1)	0.111	0.000	0.000	0.001	0.001	0.000	0.380	0.000	0.000	0.000	0.001	0.000
	(0.019)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.066)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel B: Reduced Form												
ΔQuota_m (ρ_1)	0.004	0.004	0.004	0.004	0.004	0.004	0.008	0.008	0.008	0.008	0.008	0.008
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
RF wild-cluster p-value	0.420	0.419	0.425	0.421	0.421	0.417	0.023	0.024	0.023	0.023	0.023	0.024
Panel C: Two-Stage Least Squares												
$\widehat{D}_m$ (β_1)	0.037	8.480	10.276	6.844	8.241	14.141	0.020	24.584	19.261	15.579	13.966	18.877
	(0.040)	(9.183)	(11.338)	(7.957)	(9.318)	(14.656)	(0.009)	(11.629)	(9.830)	(7.886)	(5.150)	(7.715)
Kleibergen–Paap F	34.65	15.99	13.03	10.40	10.80	11.02	33.13	16.01	12.29	15.75	27.46	21.32
AR 95% CI	[-0.038, 0.136]	[-9.285, 34.232]	[-19.448, 26.142]	[-8.076, 31.812]	[-16.955, 21.431]	[-30.666, 34.424]	[0.005, 0.045]	[6.520, 58.249]	[4.697, 55.719]	[3.080, 49.813]	[3.835, 27.785]	[5.558, 36.971]
Municipalities	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156	4,156
State Clusters	26	26	26	26	26	26	26	26	26	26	26	26

Notes: The continuous column reproduces this paper's Project Full Column (6). The other columns replace the endogenous BF level with a post-period indicator for any realized increase or an increase at or above the unweighted P25, P50, P75, or P90 of the common municipality sample. The excluded quota shock remains continuous in every column. Thresholds are computed separately by horizon and scale, then held fixed across weighted and unweighted tables. The binary coefficients are vote-share percentage-point effects of an instrument-induced change from zero to one in the probability of crossing the displayed realized-expansion margin; they are not unit conversions of the continuous coefficient and need not identify the same complier-weighted effect. The thresholds use realized post-reform treatment changes and therefore define diagnostic estimands rather than predetermined subgroups. Every column uses Project Full: municipality and year fixed effects, 2008 winning-party-by-post fixed effects, realized 2008 BF intensity, municipality characteristics, and mayor characteristics. State-by-post fixed effects, Gerard–Naritomi–Silva controls, and the post-treatment exclusion-channel correction are omitted. Every regression uses 2006-population weights. The instrument and endogenous variable are each divided by 2006 population and multiplied by 10,000. Thus π_1 is the change in realized families per 10,000 residents per one-unit quota-rate shock; ρ_1 is the change in incumbent vote-share percentage points per one-unit quota-rate shock; and β_1 is the change in vote-share percentage points per one additional realized beneficiary family per 10,000 residents. Reduced-form p-values use the wild-cluster bootstrap by state. Blue 2SLS cells indicate that the exact state-clustered Anderson–Rubin 95% set excludes zero; blue reduced-form cells use the wild-cluster p-value, and blue first-stage cells use the conventional state-clustered p-value. Disconnected AR sets are displayed conservatively as $[-\infty,+\infty]$, with exact components in the sidecar.

Table C.38: Predetermined Municipal Heterogeneity, Unweighted Raw Levels: Vote Share

Group	Municipalities (1)	KP F (2)	RF (3)	RF wild p (4)	2SLS (SE) (5)	Equality p (6)	AR 95% CI (7)
Panel A: 12-month design through 2009							
Full sample	2,078	1.56	0.67	0.095	1.85 (1.57)	—	$[-\infty, +\infty]$
Baseline BF intensity: at or below median	1,039	1.51	0.51	0.358	1.21 (1.45)	—	$[-\infty, +\infty]$
Baseline BF intensity: above median	1,039	0.59	-0.27	0.777	-16.85 (61.33)	0.772	$[-\infty, +\infty]$
Urban population share: at or below median	1,039	10.95	1.08	0.436	15.07 (14.46)	—	[-34.16, 44.70]
Urban population share: above median	1,039	1.54	0.38	0.247	0.98 (1.01)	0.331	$[-\infty, +\infty]$
Gini coefficient: at or below median	1,128	4.70	1.27	0.060	3.93 (3.07)	—	[-0.16, 104.86]
Gini coefficient: above median	950	0.98	0.41	0.518	0.83 (1.11)	0.352	$[-\infty, +\infty]$
Illiteracy rate: at or below median	1,040	1.51	0.59	0.291	1.35 (1.47)	—	$[-\infty, +\infty]$
Illiteracy rate: above median	1,038	0.01	-0.07	0.931	-19.31 (381.75)	0.957	$[-\infty, +\infty]$
Panel B: 12-month design through 2012							
Full sample	2,078	45.79	0.18	0.203	0.33 (0.14)	—	[0.09, 0.74]
Baseline BF intensity: at or below median	1,039	55.81	0.17	0.223	0.30 (0.15)	—	[0.06, 0.70]
Baseline BF intensity: above median	1,039	4.36	-0.63	0.543	-2.33 (3.01)	0.395	[-102.01, 11.37]
Urban population share: at or below median	1,039	18.39	0.96	0.432	2.00 (2.20)	—	[-3.33, 7.02]
Urban population share: above median	1,039	48.98	0.15	0.115	0.27 (0.11)	0.437	[0.08, 0.59]
Gini coefficient: at or below median	1,128	38.03	1.20	0.051	3.70 (1.79)	—	[0.79, 8.96]
Gini coefficient: above median	950	68.33	0.11	0.390	0.21 (0.05)	0.061	[0.10, 0.32]
Illiteracy rate: at or below median	1,040	57.95	0.17	0.163	0.30 (0.13)	—	[0.07, 0.66]
Illiteracy rate: above median	1,038	0.73	-0.25	0.835	-1.71 (9.44)	0.833	$[-\infty, +\infty]$

Notes: Municipalities are split once at the unweighted median of four predetermined characteristics: realized BF-family intensity in 2008, Census-2000 urban population share, the Census-2000 Gini coefficient, and the Census-2000 illiteracy rate among people aged 15 and older. Group definitions are fixed across horizons, scales, and weighting schemes. Every row retains the continuous endogenous BF measure, continuous quota instrument, and Project Full specification with municipality and year fixed effects. State-by-post fixed effects and post-treatment channel controls are omitted. Regressions are unweighted. The instrument and endogenous variable are each divided by 1,000. Thus π_1 is the change in thousands of realized beneficiary families per one-thousand-family quota shock; ρ_1 is the change in incumbent vote-share percentage points per one-thousand-family quota shock; and β_1 is the change in vote-share percentage points per 1,000 additional realized beneficiary families. Reduced-form p-values use the state wild-cluster bootstrap. Blue 2SLS cells indicate exact AR zero exclusion. Equality p-values come from pooled two-endogenous-variable 2SLS models that allow every nuisance term to differ across the two groups. They use the state-clustered Wald covariance but are not weak-IV robust, so the split-sample estimates diagnose scope and support and the equality test is secondary evidence rather than proof of causal moderation.

Table C.39: Predetermined Municipal Heterogeneity, Population-Weighted Raw Levels: Vote Share

Group	Municipalities (1)	KP F (2)	RF (3)	RF wild p (4)	2SLS (SE) (5)	Equality p (6)	AR 95% CI (7)
Panel A: 12-month design through 2009							
Full sample	2,078	32.72	0.96	0.069	0.92 (0.38)	—	[0.17, 1.79]
Baseline BF intensity: at or below median	1,039	28.78	0.81	0.205	0.80 (0.49)	—	[-0.20, 2.03]
Baseline BF intensity: above median	1,039	1.00	-0.17	0.806	-6.61 (21.68)	0.738	$[-\infty, +\infty]$
Urban population share: at or below median	1,039	10.61	1.95	0.218	21.19 (7.68)	—	[-0.36, 40.67]
Urban population share: above median	1,039	30.88	0.98	0.067	0.99 (0.45)	0.061	[0.11, 2.20]
Gini coefficient: at or below median	1,128	9.12	1.21	0.169	2.21 (1.15)	—	[0.51, 8.43]
Gini coefficient: above median	950	40.02	0.86	0.103	0.79 (0.44)	0.214	[-0.11, 1.75]
Illiteracy rate: at or below median	1,040	29.47	0.90	0.188	0.90 (0.51)	—	[-0.22, 2.14]
Illiteracy rate: above median	1,038	0.10	-0.58	0.619	38.83 (94.10)	0.698	$[-\infty, +\infty]$
Panel B: 12-month design through 2012							
Full sample	2,078	370.32	0.15	0.062	0.27 (0.13)	—	[0.04, 0.51]
Baseline BF intensity: at or below median	1,039	826.87	0.13	0.110	0.23 (0.13)	—	[-0.03, 0.52]
Baseline BF intensity: above median	1,039	4.67	-0.66	0.408	-1.88 (1.79)	0.231	[-8.57, 14.43]
Urban population share: at or below median	1,039	20.62	2.45	0.437	4.37 (2.07)	—	[-1.37, 8.37]
Urban population share: above median	1,039	465.82	0.16	0.066	0.29 (0.14)	0.050	[0.02, 0.59]
Gini coefficient: at or below median	1,128	45.62	1.01	0.063	3.27 (1.37)	—	[1.00, 7.16]
Gini coefficient: above median	950	486.48	0.14	0.151	0.25 (0.13)	0.031	[-0.01, 0.54]
Illiteracy rate: at or below median	1,040	419.52	0.16	0.065	0.27 (0.14)	—	[0.00, 0.58]
Illiteracy rate: above median	1,038	0.03	-0.62	0.693	-16.89 (135.83)	0.901	$[-\infty, +\infty]$

Notes: Municipalities are split once at the unweighted median of four predetermined characteristics: realized BF-family intensity in 2008, Census-2000 urban population share, the Census-2000 Gini coefficient, and the Census-2000 illiteracy rate among people aged 15 and older. Group definitions are fixed across horizons, scales, and weighting schemes. Every row retains the continuous endogenous BF measure, continuous quota instrument, and Project Full specification with municipality and year fixed effects. State-by-post fixed effects and post-treatment channel controls are omitted. Regressions use 2006-population weights within each displayed group. The instrument and endogenous variable are each divided by 1,000. Thus π_1 is the change in thousands of realized beneficiary families per one-thousand-family quota shock; ρ_1 is the change in incumbent vote-share percentage points per one-thousand-family quota shock; and β_1 is the change in vote-share percentage points per 1,000 additional realized beneficiary families. Reduced-form p-values use the state wild-cluster bootstrap. Blue 2SLS cells indicate exact AR zero exclusion. Equality p-values come from pooled two-endogenous-variable 2SLS models that allow every nuisance term to differ across the two groups. They use the state-clustered Wald covariance but are not weak-IV robust, so the split-sample estimates diagnose scope and support and the equality test is secondary evidence rather than proof of causal moderation.

Table C.40: Predetermined Municipal Heterogeneity, Unweighted Rates per 10,000: Vote Share

Group	Municipalities (1)	KP F (2)	RF (3)	RF wild p (4)	2SLS (SE) (5)	Equality p (6)	AR 95% CI (7)
Panel A: 12-month design through 2009							
Full sample	2,078	79.29	-0.001	0.433	-0.009 (0.011)	—	[-0.033, 0.015]
Baseline BF intensity: at or below median	1,039	58.19	-0.006	0.033	-0.045 (0.017)	—	[-0.085, -0.010]
Baseline BF intensity: above median	1,039	42.82	0.000	0.972	0.000 (0.013)	0.023	[-0.025, 0.030]
Urban population share: at or below median	1,039	51.55	-0.001	0.749	-0.005 (0.015)	—	[-0.037, 0.026]
Urban population share: above median	1,039	103.53	-0.005	0.102	-0.040 (0.019)	0.171	[-0.080, 0.002]
Gini coefficient: at or below median	1,128	67.93	0.000	0.951	0.001 (0.019)	—	[-0.041, 0.039]
Gini coefficient: above median	950	61.04	-0.002	0.364	-0.017 (0.018)	0.514	[-0.058, 0.018]
Illiteracy rate: at or below median	1,040	50.15	-0.005	0.095	-0.043 (0.020)	—	[-0.083, 0.004]
Illiteracy rate: above median	1,038	42.01	0.000	0.957	0.001 (0.012)	0.053	[-0.026, 0.028]
Panel B: 12-month design through 2012							
Full sample	2,078	91.06	-0.001	0.623	-0.002 (0.003)	—	[-0.008, 0.005]
Baseline BF intensity: at or below median	1,039	78.17	-0.003	0.361	-0.007 (0.007)	—	[-0.021, 0.007]
Baseline BF intensity: above median	1,039	80.97	0.000	0.928	0.000 (0.003)	0.384	[-0.007, 0.006]
Urban population share: at or below median	1,039	83.09	0.000	0.996	0.000 (0.003)	—	[-0.007, 0.007]
Urban population share: above median	1,039	82.50	-0.005	0.095	-0.011 (0.006)	0.121	[-0.022, 0.001]
Gini coefficient: at or below median	1,128	95.09	0.000	0.998	0.000 (0.006)	—	[-0.013, 0.013]
Gini coefficient: above median	950	60.36	-0.001	0.501	-0.003 (0.004)	0.737	[-0.011, 0.004]
Illiteracy rate: at or below median	1,040	35.05	-0.003	0.198	-0.009 (0.006)	—	[-0.020, 0.006]
Illiteracy rate: above median	1,038	101.46	0.000	0.987	0.000 (0.003)	0.197	[-0.007, 0.007]

Notes: Municipalities are split once at the unweighted median of four predetermined characteristics: realized BF-family intensity in 2008, Census-2000 urban population share, the Census-2000 Gini coefficient, and the Census-2000 illiteracy rate among people aged 15 and older. Group definitions are fixed across horizons, scales, and weighting schemes. Every row retains the continuous endogenous BF measure, continuous quota instrument, and Project Full specification with municipality and year fixed effects. State-by-post fixed effects and post-treatment channel controls are omitted. Regressions are unweighted. The instrument and endogenous variable are each divided by 2006 population and multiplied by 10,000. Thus π_1 is the change in realized families per 10,000 residents per one-unit quota-rate shock; ρ_1 is the change in incumbent vote-share percentage points per one-unit quota-rate shock; and β_1 is the change in vote-share percentage points per one additional realized beneficiary family per 10,000 residents. Reduced-form p-values use the state wild-cluster bootstrap. Blue 2SLS cells indicate exact AR zero exclusion. Equality p-values come from pooled two-endogenous-variable 2SLS models that allow every nuisance term to differ across the two groups. They use the state-clustered Wald covariance but are not weak-IV robust, so the split-sample estimates diagnose scope and support and the equality test is secondary evidence rather than proof of causal moderation.

Table C.41: Predetermined Municipal Heterogeneity, Population-Weighted Rates per 10,000: Vote Share

Group	Municipalities (1)	KP F (2)	RF (3)	RF wild p (4)	2SLS (SE) (5)	Equality p (6)	AR 95% CI (7)
Panel A: 12-month design through 2009							
Full sample	2,078	34.65	0.004	0.415	0.037 (0.040)	—	[-0.038, 0.136]
Baseline BF intensity: at or below median	1,039	38.21	0.003	0.777	0.019 (0.053)	—	[-0.079, 0.159]
Baseline BF intensity: above median	1,039	24.07	-0.001	0.563	-0.008 (0.014)	0.620	[-0.039, 0.023]
Urban population share: at or below median	1,039	52.93	0.004	0.429	0.029 (0.024)	—	[-0.030, 0.075]
Urban population share: above median	1,039	14.71	0.003	0.724	0.023 (0.059)	0.948	[-0.078, 0.226]
Gini coefficient: at or below median	1,128	49.09	0.007	0.202	0.053 (0.033)	—	[-0.012, 0.129]
Gini coefficient: above median	950	12.31	0.003	0.695	0.029 (0.057)	0.733	[-0.142, 0.152]
Illiteracy rate: at or below median	1,040	16.64	0.004	0.724	0.024 (0.058)	—	[-0.082, 0.216]
Illiteracy rate: above median	1,038	37.36	0.001	0.729	0.011 (0.018)	0.794	[-0.040, 0.042]
Panel B: 12-month design through 2012							
Full sample	2,078	33.13	0.008	0.025	0.020 (0.009)	—	[0.005, 0.045]
Baseline BF intensity: at or below median	1,039	35.28	0.015	0.048	0.045 (0.018)	—	[0.008, 0.089]
Baseline BF intensity: above median	1,039	59.08	-0.002	0.397	-0.003 (0.004)	0.022	[-0.010, 0.005]
Urban population share: at or below median	1,039	63.42	0.003	0.298	0.005 (0.004)	—	[-0.004, 0.013]
Urban population share: above median	1,039	18.78	0.012	0.021	0.042 (0.018)	0.044	[0.011, 0.103]
Gini coefficient: at or below median	1,128	41.02	0.013	0.077	0.032 (0.016)	—	[0.003, 0.073]
Gini coefficient: above median	950	16.19	0.005	0.203	0.013 (0.008)	0.333	[-0.003, 0.037]
Illiteracy rate: at or below median	1,040	17.63	0.015	0.036	0.047 (0.022)	—	[0.007, 0.118]
Illiteracy rate: above median	1,038	72.74	0.000	0.860	0.001 (0.005)	0.075	[-0.011, 0.010]

Notes: Municipalities are split once at the unweighted median of four predetermined characteristics: realized BF-family intensity in 2008, Census-2000 urban population share, the Census-2000 Gini coefficient, and the Census-2000 illiteracy rate among people aged 15 and older. Group definitions are fixed across horizons, scales, and weighting schemes. Every row retains the continuous endogenous BF measure, continuous quota instrument, and Project Full specification with municipality and year fixed effects. State-by-post fixed effects and post-treatment channel controls are omitted. Regressions use 2006-population weights within each displayed group. The instrument and endogenous variable are each divided by 2006 population and multiplied by 10,000. Thus π_1 is the change in realized families per 10,000 residents per one-unit quota-rate shock; ρ_1 is the change in incumbent vote-share percentage points per one-unit quota-rate shock; and β_1 is the change in vote-share percentage points per one additional realized beneficiary family per 10,000 residents. Reduced-form p-values use the state wild-cluster bootstrap. Blue 2SLS cells indicate exact AR zero exclusion. Equality p-values come from pooled two-endogenous-variable 2SLS models that allow every nuisance term to differ across the two groups. They use the state-clustered Wald covariance but are not weak-IV robust, so the split-sample estimates diagnose scope and support and the equality test is secondary evidence rather than proof of causal moderation.

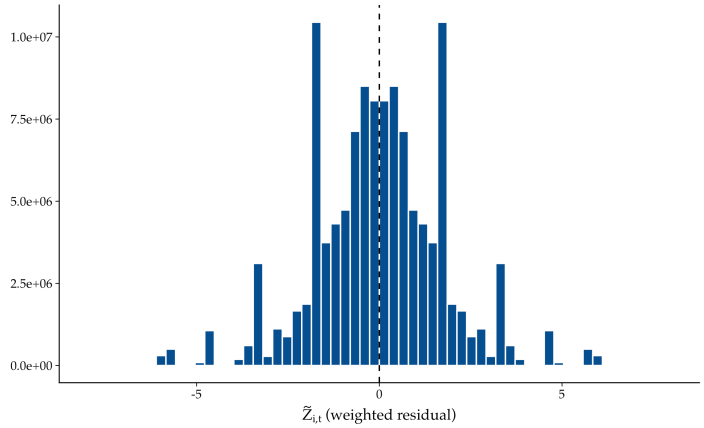
D Heterogeneity Exhibits

Table D.1: Demand-Slack Heterogeneity: Robustness to the Saturation Cut

	Slack subgroup			Saturated subgroup			
Cut definition	$\hat{\beta}_1$ (SE)	KP F	AR 95% CI	$\hat{\beta}_1$ (SE)	KP F	AR 95% CI	Wald p
Panel A. Vote share							
$\text{sat}_m \leq Q_1$	6.557 (4.057)	166	[0.90, 5.60]	37.925 (57.230)	1	[-0.85, 25.00]	0.557
$\text{sat}_m \leq$ Median	5.849 (1.656)	73	[2.35, 9.30]	68.545 (39.126)	3	[-15.00, 25.00]	0.102
$\text{sat}_m \leq$ Mean	5.419 (1.536)	70	[2.40, 9.00]	64.061 (41.298)	8	[-15.00, 25.00]	0.148
$\text{sat}_m \leq Q_3$	4.899 (1.347)	75	[2.40, 8.40]	130.645 (65.200)	7	[-15.00, 25.00]	0.053
Outside Frey 2019 cell	5.371 (1.513)	74	[2.40, 8.50]	-244.924 (185.679)	3	[-15.00, 25.00]	0.178
Panel B. Winning probability							
$\text{sat}_m \leq Q_1$	6.642 (4.940)	166	[0.40, 7.95]	24.852 (74.235)	1	[-15.00, 25.00]	0.794
$\text{sat}_m \leq$ Median	7.882 (2.187)	73	[1.50, 12.15]	164.509 (88.866)	3	[-15.00, 25.00]	0.079
$\text{sat}_m \leq$ Mean	6.353 (1.962)	70	[1.95, 10.60]	91.075 (83.465)	8	[-15.00, 25.00]	0.312
$\text{sat}_m \leq Q_3$	5.571 (1.955)	75	[1.85, 10.00]	230.821 (200.653)	7	[-15.00, 25.00]	0.263
Outside Frey 2019 cell	6.621 (2.078)	74	[1.85, 10.10]	-576.312 (461.621)	3	[-15.00, 25.00]	0.207

Notes: Each row reports a single full-sample 2SLS regression in which the instrument and the endogenous variable are interacted with a binary slack indicator and its complement, producing two endogenous coefficients in one specification. Rows 1–4 use direct cuts of $\text{sat}_m = \text{Beneficiaries}_m^{2008}/\text{pop}_m^{2000} \times 100$ (beneficiaries per 100 inhabitants in 2008), with cut points computed on the Brazil universe of municipalities for which the underlying variables are observed: $Q_1 = 5.04$, Median $= 8.37$, Mean $= 9.10$, $Q_3 = 13.33$. Row 5 uses Frey (2019)'s anchor cell (population in 2000 below 30,000 and Atlas IDHM 2010 below 0.7) as the saturation indicator, with the slack subgroup being the Frey-cell complement. All specifications use the preferred Section 4 controls: incumbent-party fixed effects based on the pre-reform winner and baseline characteristics $\times$ post (log population, Gini inequality, share of poor, household income per capita, share of formal workers, urban share). Weights: municipal population in 2000. Cluster-robust standard errors at the state level in parentheses. The Wald p-column tests $H_0 : \beta_1^{\text{slack}} = \beta_1^{\text{saturated}}$. First-stage Kleibergen-Paap F is the squared t-statistic of the own-instrument coefficient in the auxiliary first stage (same convention as Tables 15 and 19).

Figure D.1: Distribution of residualized $\tilde{Z}_m$



Note. Empirical density of the residualized quota shock interacted with Post_t under the retained 2009 compatibility design. The producer partials out its baseline-by-post controls and fixed effects using population-2000 weights. These are not the weights or sample of the current headline design. Sample: $N = 4{,}210$ municipality-year observations on $2{,}105$ municipalities. Source: the Stage 3 identifying-variation producer and its numeric sidecar, Block A.

Table D.2: Identifying weight by structural-margin subgroup

Subgroup	ω_g (weight share)	population share	municipality share
Without CRAS in 2008	0.07	0.12	0.33
With CRAS in 2008	0.93	0.88	0.67
Within CRAS: de facto turnover	0.95 (of pool)	0.89	0.84
Within CRAS: de jure turnover	0.91 (of pool)	0.85	0.79

Note. Frisch–Waugh–Lovell shares are derived from the residualized instrument–treatment covariance under the compatibility 2009 raw-level R$90/137 specification. Population and municipality shares use the full sample for the CRAS partition and the relevant observed-turnover pool for within-CRAS rows. De facto turnover is a change in the named CRAS agent between the 2008 and 2009 Censo SUAS waves; de jure turnover uses formal coordinators. Source: `3-robustness/output/datasets/d-identifying-variation-results.csv`, Block C.

Table D.3: Subgroup Decomposition of the Headline 2SLS via Frisch-Waugh-Lovell

Partition	N munis	β_g	ω_g	β_h	ω_h
Structural margins (*augmented model*)					
CRAS vs no-CRAS	1415 / 690	+5.378	0.926	+0.120	0.074
Turnover vs no-turnover (within CRAS)	1089 / 203	+5.716	0.947	+0.950	0.053
In-Frey vs out-Frey	1263 / 842	+0.848	0.147	+5.704	0.853
Q1 (low sat) vs Rest	521 / 1584	+3.428	0.664	+8.069	0.336
Other partitions (*orthogonal or unmodeled*)					
Northeast vs Other	712 / 1393	+7.898	0.208	+4.223	0.792
PT vs non-PT incumbent	259 / 1846	+4.310	0.229	+5.190	0.771
Positive vs Negative shock	1176 / 929	+3.483	0.526	+6.657	0.474

Notes: Each row reports the exact Frisch-Waugh-Lovell decomposition of the compatibility-design 2SLS estimate ($\beta = +4.99$) into a weighted average of subgroup-implied marginal effects, $\beta_{\text{full}} = \omega_g\beta_g + \omega_h\beta_h$. Weights $\omega_g = (\sum_{i\in g} w_i\tilde{z}_i\tilde{d}_i)/(\sum_i w_i\tilde{z}_i\tilde{d}_i)$ where $\tilde{z}, \tilde{d}$ are residualized against all controls and fixed effects. The decomposition holds exactly: numerical check confirms $\sum_g \omega_g\beta_g$ equals the full-sample 2SLS coefficient. These algebraic weights need not be nonnegative and are not automatically causal subgroup shares. The partitions are descriptive moderators, not identified interventions.

Table D.4: Heterogeneity by Urbanization and Joint Exposure

	Baseline Urbanization			Joint Urbanization–BF Cells		
	Below Median	Above Median	Δ	High Urban / Low BF	Low Urban / High BF	Δ
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.400	0.198	0.202	0.189	0.384	-0.196
	(0.055)	(0.047)	(0.053)	(0.056)	(0.066)	(0.084)
Conditional First-Stage F	53.04	17.84		11.16	33.59	
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	0.462	2.204	-1.742	3.115	0.182	2.933
	(0.364)	(0.725)	(0.770)	(1.111)	(0.316)	(1.185)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	1.156	11.143	-9.988	16.506	0.474	16.031
	(0.860)	(5.031)	(5.059)	(7.332)	(0.779)	(7.512)
Conditional AR 95% CI	[-0.826, 2.876]	[3.079, 28.607]		[4.330, 46.714]	[-1.590, 1.902]	
Incumbent Party	✓	✓		✓	✓	
Bolsa Família Intensity	✓	✓		✓	✓	
Municipality Characteristics	✓	✓		✓	✓	
Mayor Characteristics	✓	✓		✓	✓	
Benefit Value	✓	✓		✓	✓	
FPM Transfers	✓	✓		✓	✓	
Number of Mayors	2,078	2,078	2,078	1,406	1,406	1,406
Observations	4,156	4,156	4,156	2,812	2,812	2,812

Notes: The joint-exposure block restricts the sample to the two opposite cells named in its headers; the baseline-urbanization block uses the full headline sample. Each block is one fully interacted dual-IV regression. The instrument, endogenous variable, and complete control set are allowed to vary across the complementary subgroup indicators; Δ is the first coefficient minus the second. The saturated system reproduces separate subgroup coefficients while retaining one joint model for the displayed comparison. All models use 2006-population weights and state-clustered standard errors. Contrast columns are calculated from the joint model and therefore do not receive control check marks. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). Group AR sets are plug-in conditional diagnostics that condition on the other group coefficient, not exact joint weak-IV regions.

Table D.5: Heterogeneity: PT vs. Non-PT Incumbents

	Vote Share			Winning		
	has = 1	has = 0	Δ	has = 1	has = 0	Δ
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	8.271	4.341	3.930	23.337	2.190	21.147
	(3.306)	(1.552)	(2.301)	(7.372)	(3.308)	(7.946)
Anderson-Rubin 95% CI	[3.20, 21.00]	[1.75, 8.10]	—	[12.00, 25.00]	[-3.80, 9.60]	—
Electoral Covariates	✓	✓	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: Dual-IV full-sample interaction. "has = 1" identifies municipalities whose 2008 incumbent was from the Workers' Party (PT); "has = 0" identifies all other parties. Δ tests equality of the two coefficients via the Wald statistic from the joint model.

Table D.6: Heterogeneity: Municipality Size (Above vs. Below Median Population)

	Vote Share			Winning		
	has = 1	has = 0	Δ	has = 1	has = 0	Δ
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	5.192	-549.562	554.754	6.169	-1343.941	1350.110
	(1.408)	(222.256)	(222.318)	(2.099)	(659.469)	(660.496)
Anderson-Rubin 95% CI	[2.35, 8.40]	—	—	[1.75, 9.90]	—	—
Electoral Covariates	✓	✓	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: Dual-IV full-sample interaction. "has = 1" identifies municipalities with 2000 population at or above the sample median; "has = 0" below median.

Table D.7: Heterogeneity: Northeast vs. Other Regions

	Vote Share			Winning		
	has = 1	has = 0	Δ	has = 1	has = 0	Δ
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	20.255	5.753	14.503	-51.545	2.809	-54.354
	(54.851)	(3.765)	(51.296)	(77.517)	(5.295)	(72.601)
Anderson-Rubin 95% CI	[-15.00, 25.00]	[1.80, 7.65]	—	[-15.00, 25.00]	[3.70, 9.50]	—
Electoral Covariates	✓	✓	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: Dual-IV full-sample interaction. "has = 1" identifies municipalities in the Northeast region (AL, BA, CE, MA, PB, PE, PI, RN, SE); "has = 0" identifies all other regions. The Northeast concentrates a disproportionate share of Bolsa Família beneficiaries and is where much of the identifying variation resides.

Table D.8: Heterogeneity: Pre-Treatment Poverty

	Vote Share			Winning		
	has = 1	has = 0	Δ	has = 1	has = 0	Δ
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	61.827	4.933	56.894	217.138	5.466	211.671
	(64.537)	(1.261)	(64.288)	(189.902)	(2.110)	(190.511)
Anderson-Rubin 95% CI	[-15.00, 25.00]	[2.50, 8.70]	—	[-15.00, 25.00]	[2.05, 10.65]	—
Electoral Covariates	✓	✓	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: Dual-IV full-sample interaction. "has = 1" identifies municipalities with 2000 poverty rate at or above the sample median; "has = 0" below median.

Table D.9: Heterogeneity: 2008 Electoral Competitiveness

	Vote Share			Winning		
	has = 1	has = 0	Δ	has = 1	has = 0	Δ
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	4.593	18.092	-13.500	4.851	32.857	-28.006
	(2.620)	(12.178)	(9.754)	(3.613)	(17.112)	(14.294)
Anderson-Rubin 95% CI	[0.75, 4.35]	[3.65, 25.00]	—	[-1.95, 3.80]	[8.65, 25.00]	—
Electoral Covariates	✓	✓	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: Dual-IV full-sample interaction. "has = 1" identifies municipalities whose 2008 race was decided by a margin below the sample median (tight race); "has = 0" by a larger margin (safe race).

Table D.10: Heterogeneity: Ruling-Party Vote Share as Outcome

	Party Vote Share
$\widehat{\Delta\text{Beneficiaries}}_m$ (β_1)	2.289
	(1.277)
Anderson-Rubin 95% CI	[-0.30, 5.05]
Kleibergen-Paap F	74.16
Electoral Covariates	✓
Baseline Characteristics	✓
Observations	4,210

Notes: Preferred 2SLS with the outcome replaced by the ruling-party vote share (available for all election years regardless of candidate identity). A positive coefficient indicates that the party of the 2008 incumbent benefits from Bolsa Família expansion regardless of whether the specific incumbent stands again.

E Mechanism Exhibits

Table E.1: Heterogeneity by CRAS Presence and Active Outreach

	Δ Incumbent's Vote Share			Δ Turnout		
	No CRAS	CRAS / Without Active Outreach	CRAS / With Active Outreach	No CRAS	CRAS / Without Active Outreach	CRAS / With Active Outreach
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.325	0.436	0.276	0.325	0.436	0.276
	(0.040)	(0.108)	(0.042)	(0.040)	(0.108)	(0.042)
Conditional First-Stage F	66.15	16.22	43.12	66.15	16.22	43.12
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	-0.398	0.757	2.004	-0.138	-0.234	-0.394
	(0.637)	(0.742)	(0.646)	(0.073)	(0.167)	(0.243)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-1.226	1.738	7.273	-0.425	-0.538	-1.430
	(1.944)	(1.842)	(2.573)	(0.221)	(0.437)	(0.894)
Conditional AR 95% CI	[-5.266, 3.012]	[-1.698, 7.408]	[2.400, 13.662]	[-0.900, 0.041]	[-1.965, 0.231]	[-3.487, 0.397]
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics	✓	✓	✓	✓	✓	✓
Mayor Characteristics	✓	✓	✓	✓	✓	✓
Benefit Value	✓	✓	✓	✓	✓	✓
FPM Transfers	✓	✓	✓	✓	✓	✓
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: Each outcome is estimated on the full headline sample in one fully interacted triple-IV system with three mutually exclusive cells: no 2008 *CRAS*, *CRAS* without active outreach, and *CRAS* with active outreach. The instrument, treatment, and complete control set can vary across all three cells. The two *CRAS* coefficients are identical to those from the within-*CRAS* dual-IV system; the no-*CRAS* cell is estimated as a separate block. Models use 2006-population weights and state-clustered standard errors. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). AR sets are plug-in conditional diagnostics, not exact joint weak-IV regions.

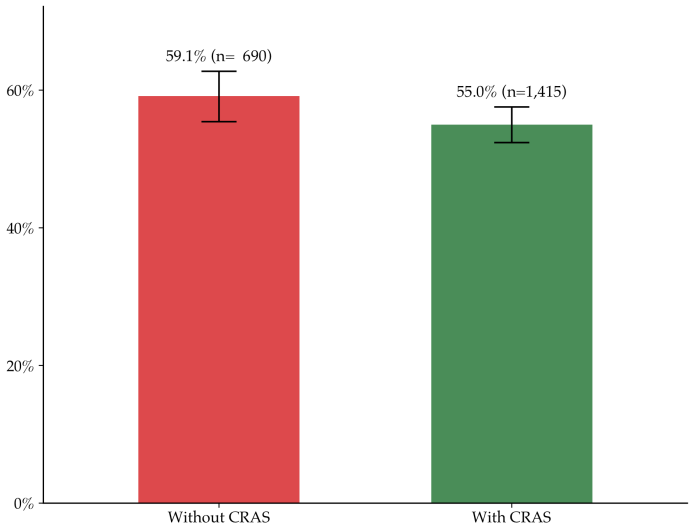


Figure E.1: 2012 incumbent win rate by CRAS presence

Notes: Bar heights report the share of 2008 incumbents winning the 2012 municipal election, separately for the 690 municipalities without CRAS and the 1415 municipalities with at least one CRAS by 2008. Error bars are 95% Wilson binomial confidence intervals. Sample: retained 2009 compatibility-design sample, not the current headline sample.

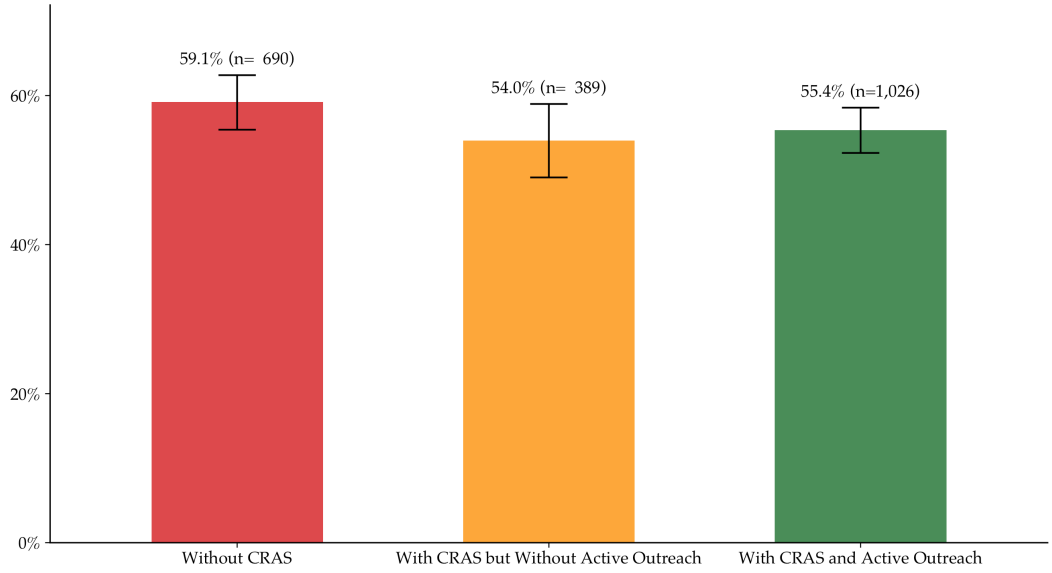


Figure E.2: 2012 incumbent win rate by active-outreach intensity

Notes: Bar heights report the share of 2008 incumbents winning the 2012 municipal election across three groups: municipalities without CRAS; municipalities with CRAS but whose CRAS does not report systematic *busca ativa*; and municipalities with CRAS that report active outreach. Error bars are 95% Wilson binomial confidence intervals. Sample: retained 2009 compatibility-design sample, not the current headline sample.

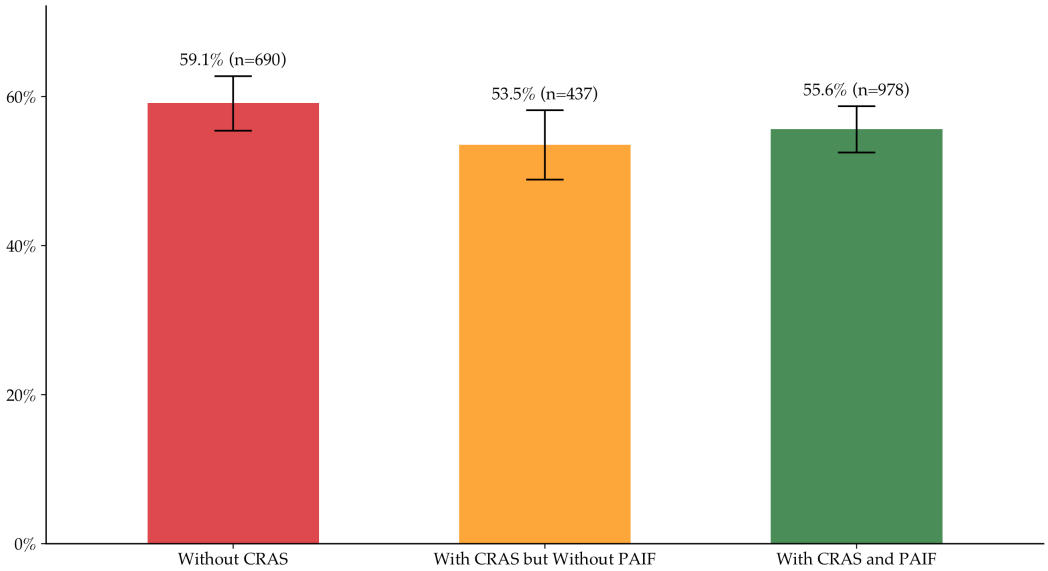


Figure E.3: 2012 incumbent win rate by PAIF-tracking intensity

Notes: Bar heights report the share of 2008 incumbents winning the 2012 municipal election across three groups: municipalities without CRAS; municipalities with CRAS that do not report tracking Bolsa Família families through PAIF; and municipalities with CRAS that do. Error bars are 95% Wilson binomial confidence intervals. Sample: retained 2009 compatibility-design sample, not the current headline sample.

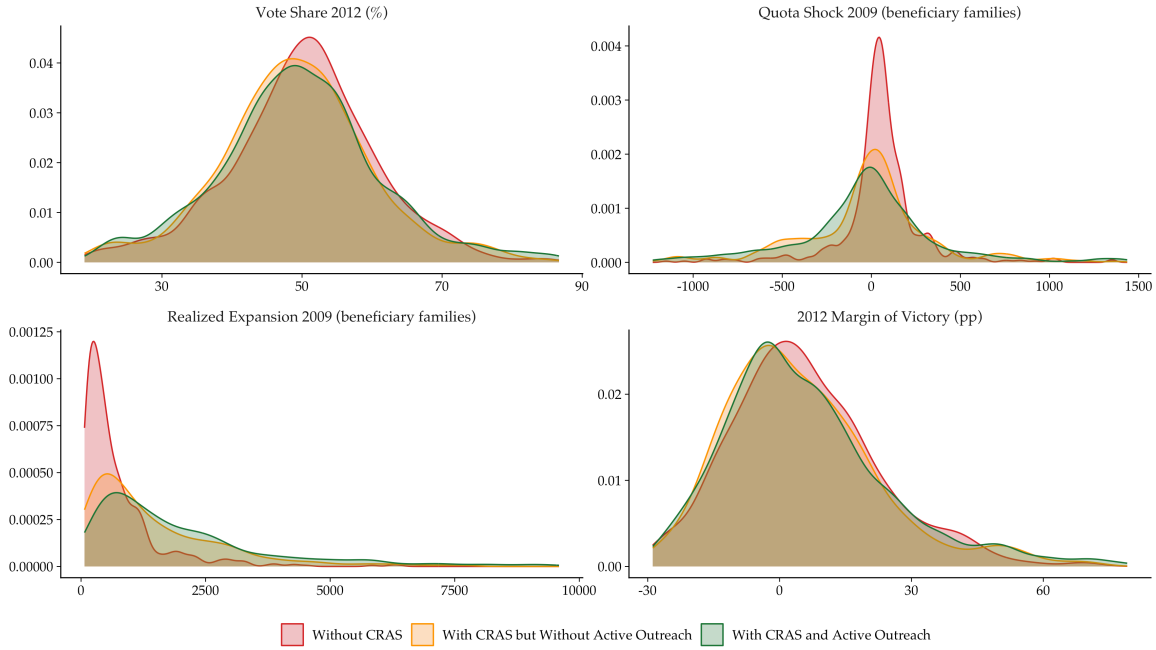


Figure E.4: Core variables by active-outreach group

Notes: Kernel densities of the 2012 incumbent vote share, the 2009 quota shock (counterfactual-based), the 12-month moving average of realized expansion centered on December 2009, and the 2012 incumbent margin of victory. Densities are truncated at the 2nd and 98th percentiles of each variable. Sample: retained 2009 compatibility-design sample, not the current headline sample.

Table E.2: Heterogeneity by CRAS Presence and PAIF Intensity

	Δ Incumbent's Vote Share			Δ Turnout		
	No CRAS (1)	CRAS / Without PAIF (2)	CRAS / With PAIF (3)	No CRAS (4)	CRAS / Without PAIF (5)	CRAS / With PAIF (6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.325	0.372	0.292	0.325	0.372	0.292
	(0.040)	(0.130)	(0.047)	(0.040)	(0.130)	(0.047)
Conditional First-Stage F	66.13	8.18	38.03	66.13	8.18	38.03
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	-0.398	0.653	2.251	-0.138	-0.442	-0.405
	(0.637)	(0.570)	(0.791)	(0.073)	(0.279)	(0.204)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-1.226	1.758	7.697	-0.425	-1.189	-1.385
	(1.944)	(1.695)	(3.104)	(0.221)	(0.933)	(0.738)
Conditional AR 95% CI	[-5.267, 3.013]	[-1.502, 9.527]	[2.010, 15.784]	[-0.900, 0.041]	[-6.262, 0.324]	[-3.195, 0.050]
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics	✓	✓	✓	✓	✓	✓
Mayor Characteristics	✓	✓	✓	✓	✓	✓
Benefit Value	✓	✓	✓	✓	✓	✓
FPM Transfers	✓	✓	✓	✓	✓	✓
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: Each outcome is estimated on the full headline sample in one fully interacted triple-IV system with three mutually exclusive cells: no 2008 *CRAS*, *CRAS* without PAIF-tracked *Bolsa Família* families, and *CRAS* with PAIF-tracked families. The instrument, treatment, and complete control set can vary across all three cells. The two *CRAS* coefficients are identical to those from the within-*CRAS* dual-IV system; the no-*CRAS* cell is estimated as a separate block. Models use 2006-population weights and state-clustered standard errors. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). AR sets are plug-in conditional diagnostics, not exact joint weak-IV regions.

Table E.3: Heterogeneity by CRAS Presence and Manager Turnover

	Δ Incumbent's Vote Share			Δ Turnout		
	No CRAS (1)	CRAS / No Observed Turnover (2)	CRAS / Observed Turnover (3)	No CRAS (4)	CRAS / No Observed Turnover (5)	CRAS / Observed Turnover (6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.325	0.264	0.311	0.325	0.264	0.311
	(0.040)	(0.054)	(0.067)	(0.040)	(0.054)	(0.067)
Conditional First-Stage F	66.18	24.22	21.59	66.18	24.22	21.59
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	-0.398	0.809	1.844	-0.138	-0.655	-0.350
	(0.637)	(0.858)	(0.657)	(0.073)	(0.343)	(0.190)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	-1.226	3.061	5.922	-0.425	-2.478	-1.126
	(1.943)	(3.202)	(2.613)	(0.221)	(1.173)	(0.698)
Conditional AR 95% CI	[-5.265, 3.011]	[-4.113, 10.410]	[1.441, 13.965]	[-0.899, 0.041]	[-5.080, 0.240]	[-3.183, 0.123]
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics	✓	✓	✓	✓	✓	✓
Mayor Characteristics	✓	✓	✓	✓	✓	✓
Benefit Value	✓	✓	✓	✓	✓	✓
FPM Transfers	✓	✓	✓	✓	✓	✓
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: Each outcome is estimated on the full headline sample in one fully interacted triple-IV system with three mutually exclusive cells: no 2008 *CRAS*, *CRAS* with no observed manager turnover, and *CRAS* with observed turnover. The no-observed-turnover cell includes both confirmed nonchanges and municipalities without a matchable 2008–2009 manager status. No municipality is excluded based on turnover matchability. The instrument, treatment, and complete control set can vary across all three cells, and all cell-specific coefficients are estimated in one joint model. Models use 2006-population weights and state-clustered standard errors. Conditional first-stage F statistics follow Sanderson and Windmeijer (2016). AR sets are plug-in conditional diagnostics, not exact joint weak-IV regions.

Table E.4: Quota shocks and changes in the observed CRAS unit network

Outcome	Coefficient	Municipalities
New CRAS IDs observed in 2009	-0.190	1,415
	(0.082)	
Share of 2009 units with new IDs (pp)	-1.171	1,401
	(0.464)	
Net increase in CRAS unit count (pp)	-2.008	1,415
	(1.770)	

Separate cross-sectional reduced forms among rerunning-mayor municipalities with a CRAS observed in 2008. The compatibility 2009 R$90/137 quota shock uses the central raw-level standardization, Census 2000 population weights, baseline controls and state fixed effects. Standard errors are clustered by state. This is not the headline 2012 rate specification. Clean unit counts come from the 2008–2009 Censo SUAS ID comparison. A new ID can reflect relabeling, not an opening; the last row measures a positive net count change, not gross entry. The share excludes municipalities without an observed 2009 unit. A statistically imprecise response does not establish no infrastructure response or identify a manager-substitution mechanism.

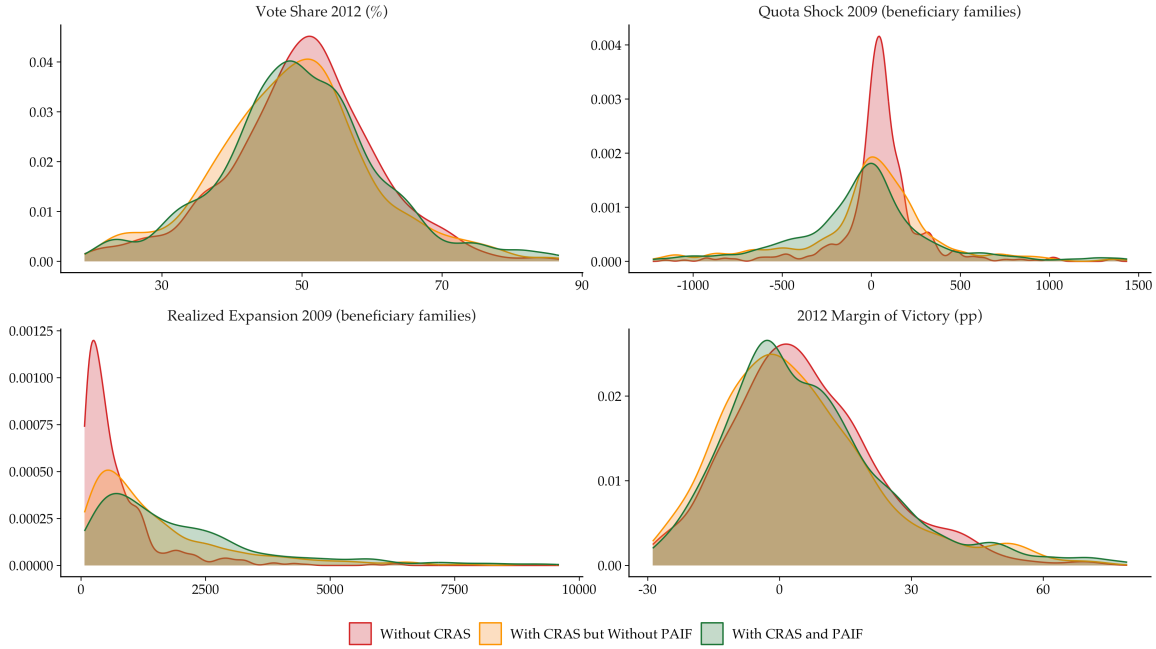


Figure E.5: Core variables by PAIF-tracking group

Notes: Kernel densities of the 2012 incumbent vote share, the 2009 quota shock (counterfactual-based), the 12-month moving average of realized expansion centered on December 2009, and the 2012 incumbent margin of victory. Densities are truncated at the 2nd and 98th percentiles of each variable. Sample: retained 2009 compatibility-design sample, not the current headline sample.

Table E.5: Heterogeneity by Continuous CRAS Capacity

	Δ Incumbent's Vote Share			Δ Turnout		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
ΔQuota_m at One CRAS	0.327	0.381	0.313	0.327	0.381	0.313
	(0.066)	(0.062)	(0.052)	(0.066)	(0.062)	(0.052)
ΔQuota_m per Additional CRAS	0.197	0.221	0.218	0.197	0.221	0.218
	(0.099)	(0.092)	(0.089)	(0.099)	(0.092)	(0.089)
Own-Instrument F at One CRAS	24.76	37.44	35.62	24.76	37.44	35.62
Own-Instrument F per Additional CRAS	3.97	5.74	5.97	3.97	5.74	5.97
Panel B: Reduced Form						
ΔQuota_m at One CRAS	0.630	0.517	0.598	-0.471	-0.382	-0.375
	(0.410)	(0.342)	(0.374)	(0.217)	(0.195)	(0.189)
ΔQuota_m per Additional CRAS	1.041	1.300	1.373	0.002	0.006	0.015
	(0.223)	(0.274)	(0.304)	(0.021)	(0.049)	(0.048)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ at One CRAS	5.668	4.048	6.172	-1.451	-1.003	-1.169
	(3.826)	(3.122)	(3.979)	(0.741)	(0.528)	(0.648)
$\Delta\widehat{\text{Beneficiaries}}_m$ per Additional CRAS	5.386	5.992	6.456	-0.013	0.001	0.042
	(2.518)	(2.355)	(2.325)	(0.126)	(0.242)	(0.245)
Conditional AR 95% CI at One CRAS	[0.226, 15.529]	[-0.519, 11.298]	[0.345, 15.500]	[-3.698, -0.078]	[-2.333, 0.049]	[-2.799, 0.078]
Conditional AR 95% CI per Additional CRAS	$[-\infty, +\infty]$	[3.148, 36.371]	[3.524, 32.266]	$[-\infty, +\infty]$	[-1.524, 0.631]	[-1.061, 0.829]
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics		✓	✓		✓	✓
Mayor Characteristics		✓	✓		✓	✓
Benefit Value			✓			✓
FPM Transfers			✓			✓
Number of Mayors	2,078	2,078	2,078	2,078	2,078	2,078
Observations	4,156	4,156	4,156	4,156	4,156	4,156

Notes: The model interacts the quota shock and beneficiary change with $n_m^{CRAS} - 1$. The first coefficient is therefore evaluated at one 2008 CRAS; the second is the marginal change associated with each additional CRAS. Each outcome uses the three headline control columns, 2006-population weights, and state-clustered standard errors.

Table E.6: Heterogeneity Within CRAS by Activity Intensity: Vote Share (Dual-IV)

	Active Outreach (*busca ativa*)			PAIF Family Registry		
	has	no	Δ	has	no	Δ
Panel A: First Stage						
ΔQuota_m (π_1)	0.209	0.187	0.022	0.209	0.174	0.035
	(0.028)	(0.004)	(0.029)	(0.030)	(0.020)	(0.036)
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	1.006	0.978	0.028	1.039	0.840	0.199
	(0.276)	(0.186)	(0.306)	(0.264)	(0.150)	(0.289)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	4.792	6.476	-1.683	4.962	5.675	-0.713
	(1.640)	(1.542)	(1.129)	(1.605)	(1.331)	(1.255)
Kleibergen-Paap F	54.63	2551.03		50.03	73.62	
AR 95% CI	[1.900, 9.050]	[3.200, 7.250]		[2.150, 9.300]	[2.900, 7.450]	
Party Fixed Effects	✓	✓	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: All estimates are obtained from a single full-sample dual-IV regression per intensity dimension: the instrument and the endogenous variable are each interacted with a redefined within-CRAS intensity indicator and its complement, producing two coefficients per equation from one regression. "has" identifies CRAS municipalities with the channel; "no" identifies CRAS municipalities without it. Municipalities lacking any CRAS carry zero on both indicators and contribute only to the estimation of fixed effects and baseline controls. Columns (1)–(3) split CRAS municipalities by whether the CRAS unit performs active outreach (*busca ativa*); columns (4)–(6) split by whether the CRAS reports any *Bolsa Família* families served under the PAIF program. Weekly interviews and home visits are omitted because over 99% of CRAS report performing both, leaving no cross-sectional variation. Δ is the Wald test of equality of the two coefficients, reported as the difference with its standard error. Layout mirrors Table 12: 90/137 quota shock instrument, year-varying realized beneficiaries (12-month MA, standardized) as the endogenous variable, party fixed effects (pre-reform winner) plus baseline characteristics × post (Census 2000 log population, share of poor, household income per capita, urban share, and 2000–2006 population growth factor), municipality and year fixed effects, weighted by 2000 population, with cluster-robust standard errors at the state level. Anderson-Rubin 95% confidence sets are computed by grid inversion holding the other group's coefficient at its dual-IV estimate.

Table E.7: Margin of Victory (2012) by CRAS-Intensity Group

Group	N	Mean	Median	SD	% Lost
Panel A: Active Outreach					
Without CRAS	690	6.08	4.01	20.58	40.7
With CRAS but Without Active Outreach	389	6.12	1.45	23.37	46.0
With CRAS and Active Outreach	1,026	7.10	2.82	23.80	44.6
Panel B: PAIF Family Registry					
Without CRAS	690	6.08	4.01	20.58	40.7
With CRAS but Without PAIF	437	5.53	1.23	22.86	46.5
With CRAS and PAIF	978	7.41	3.17	24.02	44.4

Notes: Descriptive statistics of the 2008-incumbent's 2012 margin of victory (own vote share minus runner-up's; positive = won) by CRAS-intensity group. Sample is the 2,105 municipalities in the DiD-IV estimation of Section 4. Panel A splits by active outreach (*busca ativa*); Panel B by PAIF family registry. "With CRAS but Without [Channel]" denotes municipalities with CRAS but without the indicator channel; "With CRAS and [Channel]" denotes municipalities with both. "% Lost" is the share of municipalities with negative margin (incumbent did not win reelection). The median margin is markedly smaller in the "With CRAS but Without [Channel]" group, locating these incumbents closer to the 0-pp winning threshold; this helps explain why the Vote Share vs Winning Wald tests in Table E.6 diverge in that group.

Table E.8: Separate-Subsample Heterogeneity: PAIF Family Volume within CRAS

	Vote Share			Turnout		
	Zero (1)	Below Median (2)	Above Median (3)	Zero (4)	Below Median (5)	Above Median (6)
Panel A: First Stage						
ΔQuota_m (π_1)	0.440	0.445	0.305	0.440	0.445	0.305
	(0.126)	(0.073)	(0.050)	(0.126)	(0.073)	(0.050)
Panel B: Reduced Form						
ΔQuota_m (ρ_1)	0.382	-0.004	5.185	-0.387	-0.364	-0.371
	(0.627)	(0.941)	(1.427)	(0.282)	(0.359)	(0.128)
Panel C: Two-Stage Least Squares						
$\Delta\widehat{\text{Beneficiaries}}_m$ (β_1)	0.869	-0.009	17.009	-0.878	-0.820	-1.216
	(1.460)	(2.116)	(5.763)	(0.727)	(0.781)	(0.520)
Kleibergen–Paap F	12.20	37.48	37.23	12.20	37.48	37.23
AR 95% CI	[-2.297, 5.275]	[-4.604, 4.652]	[6.783, 32.632]	[-3.531, 0.425]	[-2.465, 0.955]	[-2.651, -0.311]
Incumbent Party	✓	✓	✓	✓	✓	✓
Bolsa Família Intensity	✓	✓	✓	✓	✓	✓
Municipality Characteristics	✓	✓	✓	✓	✓	✓
Mayor Characteristics	✓	✓	✓	✓	✓	✓
Δ Benefit Value: 2012–2008	✓	✓	✓	✓	✓	✓
Number of Mayors	432	496	466	432	496	466
Observations	864	992	932	864	992	932

Table E.9: First stage by structural-margin subgroup

Subgroup	$\hat{\pi}_{FS}$	SE	First-stage F	N_{munis}
Full sample	0.198	0.023	74.2	2,105
Infrastructure margin (κ^I)*:*				
has CRAS in 2008 = 0	0.036	0.008	19.2	690
has CRAS in 2008 = 1	0.194	0.021	85.8	1,415
Alignment margin (κ^A) *within CRAS, de facto:*				
CRAS, no de facto turnover	-0.027	0.009	8.7	203
CRAS, de facto turnover	0.196	0.018	113.5	1,089
Alignment margin (κ^A) *within CRAS, de jure:*				
CRAS, no de jure turnover	-0.049	0.009	31.4	168
CRAS, de jure turnover	0.194	0.013	216.9	645

Note. Each row is a separate first-stage regression under the compatibility 2009 raw-level R$90/137 specification: municipality, year, and incumbent-party fixed effects; baseline Census-2000 characteristics interacted with post; 2000-population weights; and state-clustered standard errors. Turnover definitions match Table D.2. Within-CRAS rows need not sum to the full CRAS group because turnover is defined only for CRAS units observed in both waves. Source: `3-robustness/output/datasets/d-identifying-variation-results.csv`, Block B.

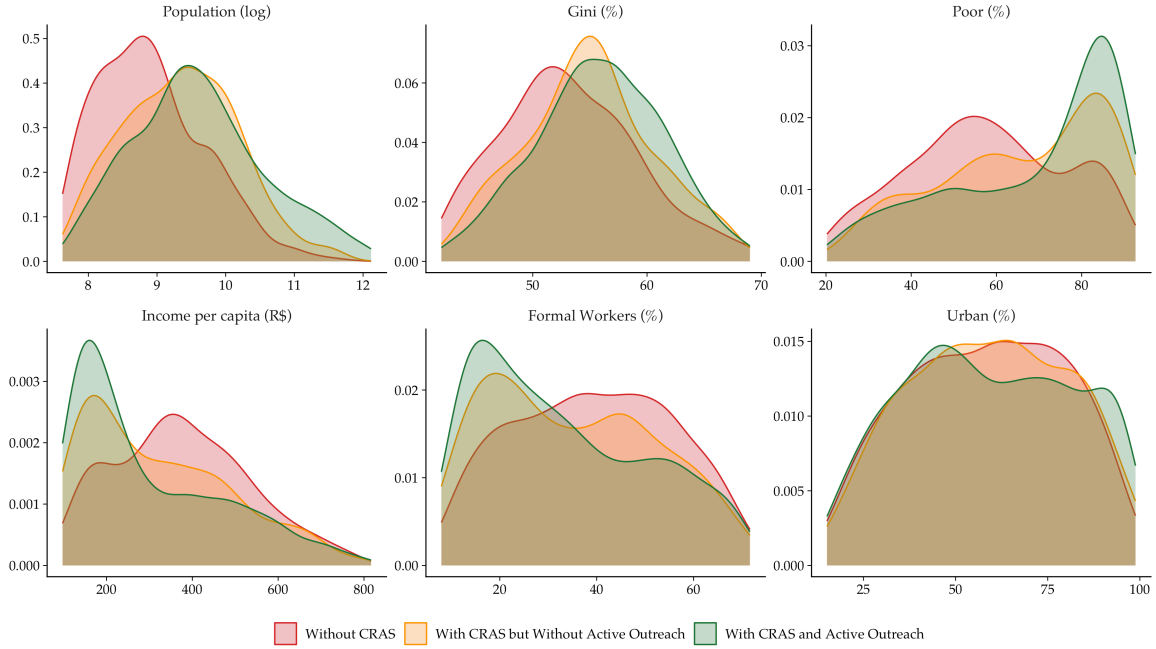


Figure E.6: Baseline covariates by active-outreach group

Notes: Kernel densities of the six Census 2000 baseline covariates displayed in the compatibility diagnostic. Densities are truncated at the 2nd and 98th percentiles of each variable. Gini and urban share are displayed multiplied by 100 to align with the other percentage-scale variables. Sample: retained 2009 compatibility-design sample, not the current headline sample.

Table E.10: Formal Within-Subgroup Identification (Three-Cell Partition)

	no-CRAS	CRAS, no turnover	CRAS, turnover
First-stage and identifying mass			
First-stage $\hat{\pi}$	0.036	-0.027	0.196
	(0.008)	(0.009)	(0.018)
Kleibergen-Paap F	19.2	8.7	113.5
FWL identifying share ω_g	0.057	0.057	0.885
Within-cell 2SLS (Spec B; endogenous in levels)			
Vote share $\hat{\beta}_g$	-154.30	-20.44	5.94
	(63.61)	(33.54)	(1.54)
AR 95% set (vote share)	$[-\infty, +\infty]$	[-15.00, 21.20]	[3.15, 9.60]
Winning $\hat{\beta}_g$	71.85	238.81	7.40
	(169.64)	(48.05)	(2.39)
AR 95% set (winning)	[unbounded]	$[-\infty, +\infty]$	[2.65, 12.65]
Municipalities	690	203	1,089
Pairwise Wald tests of $\hat{\pi}$ equality			
$\hat{\pi}_{\text{CRAS, turnover}} = \hat{\pi}_{\text{no-CRAS}}$	$\Delta = 0.160$	$z = 7.93$	$p < 0.001$
$\hat{\pi}_{\text{CRAS, turnover}} = \hat{\pi}_{\text{CRAS, no turnover}}$	$\Delta = 0.223$	$z = 10.87$	$p < 10^{-15}$
$\hat{\pi}_{\text{CRAS, no turnover}} = \hat{\pi}_{\text{no-CRAS}}$	$\Delta = -0.063$	$z = -5.13$	$p < 0.001$

Notes: Three-cell partition refining the joint pre-reform infrastructure and manager-turnover margins: *no-CRAS* are municipalities without a *CRAS* in the 2008 *Censo SUAS*; *CRAS, no turnover* are municipalities with a *CRAS* but no change in the *de facto* front-line manager between the 2008 and 2009 waves; *CRAS, turnover* are municipalities with a *CRAS* and a manager change. Each cell-level estimate is obtained from a within-cell 2SLS regression on the subsample of municipality-year observations belonging to that cell. The first stage regresses the standardized 12-month moving average of *Bolsa Família* beneficiaries on the standardized 2009 quota shock $\times$ post, controlling for the six Census 2000 baseline characteristics $\times$ post and for municipality, year, and incumbent-pre-reform-winner party fixed effects, weighted by 2000 population, with cluster-robust standard errors at the state level. The 2SLS coefficient identifies the within-cell average causal response, in the sense of Angrist and Imbens (1995), of incumbent vote share (or the probability of reelection) to instrumented beneficiary expansion within the cell. Anderson-Rubin 95% confidence sets are grid-inverted over $\beta_0 \in [-15, 25]$ with step 0.05; sets reaching either grid endpoint are reported as "[unbounded]" since the AR test does not reject any candidate β_0 within the searched grid in that cell, the appropriate inference output when the within-cell first stage is statistically indistinguishable from zero. The FWL identifying share ω_g is the share of the total residualized weighted variance of shock$_m$ $\times$ post that lies in cell g, the implicit weight cell g carries in the pooled 2SLS estimand. Pairwise Wald tests of $\hat{\pi}$ equality assume independent cell estimates and report a one-degree-of-freedom χ^2 p-value on the squared standardized difference.

Table E.11: Within-CRAS Heterogeneity by Hire Type of Manager Substitute

Panel A: Four-cell partition refining CRAS-turnover by hire type

	no-CRAS	CRAS-turn external hire	CRAS-turn internal same CRAS	CRAS-turn internal other CRAS
First-stage $\hat{\pi}$	0.036	0.090	0.198	-0.019
	(0.008)	(0.022)	(0.015)	(0.003)
Kleibergen-Paap F	19.2	17.4	186.4	34.4
Vote share $\hat{\beta}_g$	-154.30	8.08	12.53	-66.76
	(63.61)	(3.11)	(2.61)	(27.97)
AR 95% (vote share)	[unbounded]	[3.30, 19.35]	[7.45, 18.40]	[-15.00, -11.60]
Winning $\hat{\beta}_g$	71.85	23.53	15.17	-157.74
	(169.64)	(7.85)	(5.45)	(162.91)
AR 95% (winning)	[unbounded]	[2.50, 25.00]	[4.20, 25.00]	[unbounded]
Municipalities	690	861	105	109

Panel B: Two-cell partition within CRAS-turnover

	CRAS-turn external dominant	CRAS-turn internal dominant
First-stage $\hat{\pi}$	0.090	0.118
	(0.022)	(0.041)
Kleibergen-Paap F	17.4	8.2
Vote share $\hat{\beta}_g$	8.08	17.86
	(3.11)	(6.89)
AR 95% (vote share)	[3.30, 19.35]	[8.70, 25.00]
Winning $\hat{\beta}_g$	23.53	26.67
	(7.85)	(16.07)
AR 95% (winning)	[2.50, 25.00]	[3.85, 25.00]
Municipalities	861	228

Notes: Hire type of the manager substitute is classified at the CRAS level from the matched 2008–2009 *Censo SUAS* CPF panel: a change is *external* if the successor (2009 *cpf_agente*) was not present anywhere in the *CRAS* structure in 2008, *internal same CRAS* if the successor was present at the same unit in another role, and *internal other CRAS* if the successor was present at a different unit. A municipality is assigned to a cell by the dominant type of its manager changes (strict majority; "mixed" municipalities are pooled with internal in Panel B and excluded from Panel A). The headline distribution of changes across the 2,583 municipalities with at least one manager turnover is 75.5% external, 14.7% internal same CRAS, 9.8% internal other CRAS. Each cell-level estimate is obtained from a within-cell 2SLS regression with the headline specification (Column 3 of Table 9): municipality, year, and pre-reform-winner party fixed effects, baseline Census 2000 characteristics $\times$ post, weighted by 2000 population, cluster-robust standard errors at the state level. Anderson-Rubin 95% sets are grid-inverted over $\beta_0 \in [-15, 25]$.

Table E.12: Heterogeneity by Manager Turnover — *De Jure* Definition

	Vote Share			Winning		
	non-CRAS	CRAS / non-Turn	CRAS / Turnover	non-CRAS	CRAS / non-Turn	CRAS / Turnover
First Stage (π_1)	0.039	-0.031	0.235	0.039	-0.031	0.235
	(0.003)	(0.044)	(0.025)	(0.003)	(0.044)	(0.025)
Reduced Form (ρ_1)	-5.148	1.618	0.972	3.535	-0.538	1.335
	(1.981)	(2.105)	(0.194)	(7.556)	(4.621)	(0.388)
Two Stage Least Squares (β_1)			3.217			5.948
			(1.662)			(2.894)
Kleibergen-Paap F	186.85	0.51	85.86	186.85	0.51	85.86
AR 95% CI	$[-\infty, +\infty]$	$[-\infty, +\infty]$	[+0.600, +6.650]	$[-\infty, +\infty]$	$[-\infty, +\infty]$	[+2.800, +8.850]
Party Fixed Effects	✓	✓	✓	✓	✓	✓
Baseline Characteristics	✓	✓	✓	✓	✓	✓
Observations	4,210	4,210	4,210	4,210	4,210	4,210

Notes: Same three-cell partition and specification as Table 19, except the manager is defined here as the *de jure* frontline manager: the formal *Coordenador* listed on the *CRAS* RH staff file. This is a noisier proxy because a formal change of title without a change in the person who actually runs the unit does not register in the *de facto* measure used in the main text. The first-stage asymmetry is sharper than under the *de facto* measure (the no-turnover cell is even closer to zero), but the within-turnover-cell 2SLS point estimate is small and statistically indistinguishable from zero, the symptom of using a noisier turnover indicator. The augmented model's κ^A is therefore best read as a *de facto* object, with the formal-coordinator margin reported here as a noisier alternative. As in the main table, the 2SLS coefficient and Anderson-Rubin set are reported only for the CRAS / Turnover cell; the non-CRAS and CRAS / non-Turn cells are not identified, so their 2SLS estimate is left blank and their AR set is reported as unbounded ($[-\infty, +\infty]$).

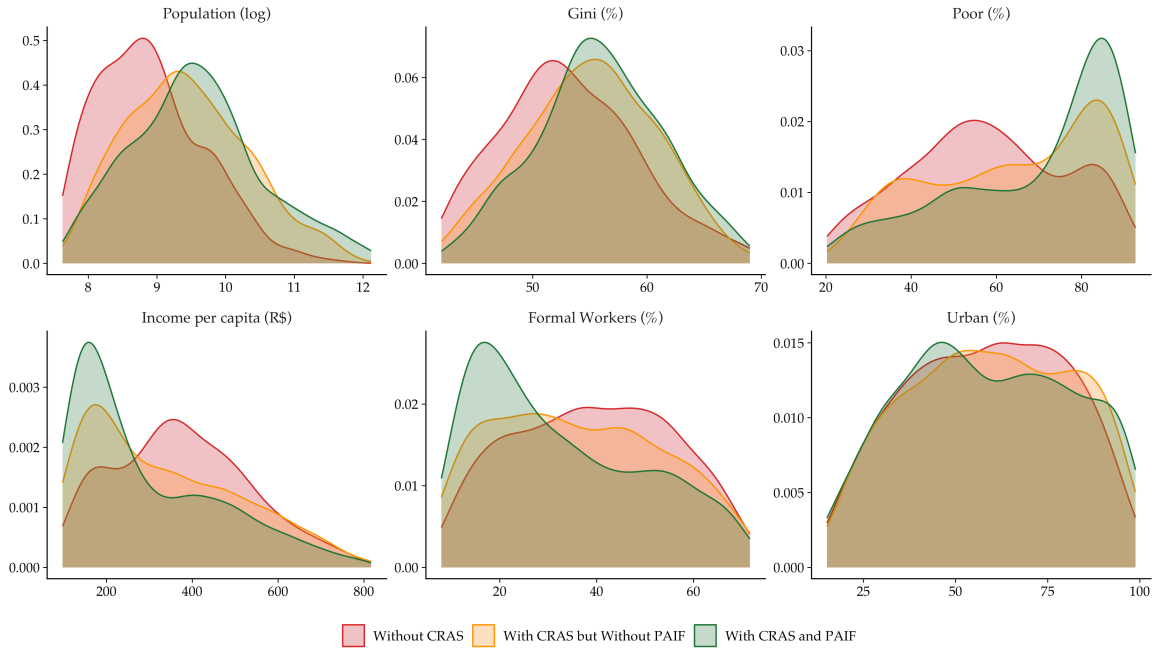


Figure E.7: Baseline covariates by PAIF-tracking group

Notes: Kernel densities of the six Census 2000 baseline covariates displayed in the compatibility diagnostic. Densities are truncated at the 2nd and 98th percentiles of each variable. Gini and urban share are displayed multiplied by 100 to align with the other percentage-scale variables. Sample: retained 2009 compatibility-design sample, not the current headline sample.